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I. THE COAL-FAMINE.

By Professor EDWARD HULL, M.A. F.R.S.

DEARTH in the midst of plenty. Such is the expression in general use with reference to coal in this year of grace, 1873. In London (as I write) the price is 50 shillings a ton; in Dublin, 40 shillings; in Belfast, something between the two. In various parts of England and Scotland the price ranges from 30 to 50 shillings, according to circumstances. The price may be considered as generally doubled all over the country; and in some districts—situated even on the borders of coal-fields themselves—it is often difficult to procure a ton unless by notice delivered to the coal-merchant several days beforehand. All classes feel the pinch, with the exceptions of colliery proprietors and coal-miners. The wealthy, of course, still keep up their fires, and pay heavily for the luxury; the middle classes, clerks with small salaries, civil-servants, curates, and professional men commencing life, are obliged to stint themselves of warmth, and find that there is much more difficulty in keeping a balance between income and expenditure than heretofore. And the poor — one may well pause to enquire how *they* manage to keep out the winter's frost and cook their little meals while every hundredweight of coal costs two shillings or half-a-crown. Christian philanthropy steps in, and by establishing coal-funds and various means of relief, helps to alleviate the distress; but many a poor widow or worn-out labourer has the ordinary privations of life aggravated fourfold by the want of a good fire — one of the few bright and cheerful things to be seen in a poor man's cottage. We commend this consideration to the attention of that mysterious authority which assumes the right of limiting the supply of coal in order to keep up the price. It may be a supreme source of gratification to have the power of crippling industry, disorganising trade, and causing a severe pressure amongst a "bloated aristocracy," but do the men who pull the wires of this secret organisation ever reflect that they are bringing misery and want to thousands of poor men's homes? To what extent the increased price of coal is bearing on the resources of the community at large is a question to which Sir W. Armstrong has attempted to give a reply. In a recent address to the North of England Institute of Mining Engineers, he states that the rise in price may be estimated as equivalent to a tax on coal to the extent of 44 millions sterling.* To manufacturers who consume on their works from 100 to 300 tons per week, the difference in price may represent the difference between profit and no profit, or even loss; and already we hear of factories about to be closed and iron furnaces "blown out," while strikes and dear coal have driven many branches of trade away from their original sites.†

Anyone arriving on our shores, and unacquainted with the course of events of the last few years, would naturally conclude that the long-threatened exhaustion of our coal-mines was actually impending; or, at any rate, that the quantity of fuel in the under-ground cellars had become so far diminished that the quantity available for supply had materially fallen off. As a matter of fact, our position is now very much what we should expect it to be if one-half of our available supply was exhausted. If, however, our visitor were informed that the coal is as plentiful as ever, that the difficulties of mining are not materially increased as compared with the last few years, that miners are numerous, and the mines on the whole only a little deeper than when coal was 20 shillings a ton in London, he might well be excused if he received such a statement with incredulity.

And yet such is in reality the case. The researches of the Royal Commissioners on Coal-Supply have fully demonstrated that there is sufficient coal within workable depth to supply the wants of the population of these countries for several centuries, even with an annual increase calculated on the rate of increase of past years. On the basis of a diminishing ratio of increase, Mr. Price Williams, whose views are quoted with approval by the Commissioners,‡ calculates that the annual consumption at the end of a century would amount to 274 millions of tons, and that the total quantity of available coal, as estimated by the Commissioners themselves, would last for 360 years. Another estimate, made on the basis of an arithmetical increase of three millions of tons per annum (the increase of the last fourteen years), would make the consumption at the end of a century amount to 415 millions of tons, and the estimated available quantity would then be only sufficient to last for 276 years. Upon both of these calculations, however, the writer has recently had occasion to remark "that they labour under the defect of not taking into account the diminishing rate at which coal must be consumed when it becomes scarcer and more expensive. The abrupt exhaustion of our coal-fields is an impossibility, and if it is to take place at all it can only be by a slow and gradual process, concomitant with a complete—let us hope a higher and nobler—reorganisation of society." *

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* Nature, No. 171, p. 271.

† It was recently stated, at a meeting of the Manchester Chamber of Commerce, that the rise in the price of coal may be considered to represent an increase of one halfpenny per pound in the price of cotton. In Lancashire the rise is less severely felt than in some other places.

‡ Report, vol. i., p. xv.

Whatever, therefore, may be the ultimate period of exhaustion, it is clear at least that it is far removed from ourselves, and we must therefore look to other causes than that of failure of supply to account for the present high price and scarcity of mineral fuel.

These causes, in our view, are twofold. First and chief, want of thrift and intelligence amongst the mining population; and secondly, interference with the free action of the law of supply and demand. Owing to the former, the miner has generally little desire to emulate the rest of the world in making money, being satisfied if by working short time he can earn sufficient to pay his way; and owing to the latter, the supply of coal is restricted in obedience to the authority of a secret tribunal which few working men have the courage to resist. It might, however, be justly said, that the power of such a tribunal over the individual actions of coal-miners, as of other workmen, is a consequence of want of intelligence on the part of the mining population, —so that the ultimate cause of the present state of things is the low state of education, of thrift, and of self-dependence amongst the working classes. Were the ordinary motives for accumulating money, for "bettering one's self," and rising in the world, prevalent amongst pitmen, and were the laws of supply and demand left to have free play in regulating quantities and prices, it might be assumed that those artificial combinations amongst workmen on the one hand, and employers on the other, which are bearing so disastrously upon the comfort and prosperity of the community, would be unknown.

In order more fully to understand the question, let us

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* Coal-Fields of Great Britain, 3rd edit. (1873), p. 454.

briefly review the origin of the present scarcity and high prices of coal.

Upon the cessation of the Franco-German war, when the great duel had been fought out, and the combatants retired within the new boundaries of their territories, a revival of trade brought with it an extraordinary demand for iron. The stocks of pig-iron accumulating in Glasgow and other markets were almost cleared off, and, as a necessary result, the smelting-furnaces all over the country were "blown in," and then sprung up a great demand for coal with which to feed them. The price accordingly went up, and doubtless the proprietors of the mines were the first to feel the benefit of the enhanced prices; but there soon followed, as was perfectly natural, a demand on the part of the miners for increased wages, which was generally acceded to; and ultimately wages increased to such an extent that with restricted time a pitman of ordinary skill can earn at a rate varying from £120 to £150 per annum, and, if he condescends to work five days in the week, considerably more.

A reaction has, however, set in; the enhanced price of iron has shortened the demand, and with this ought to come, in the ordinary course of things, a lessening of the demand for coal and a fall in prices. When notices of a reduction of wages were served on the pitmen, the result has invariably been to cause a strike, such as that we have just witnessed on a gigantic scale in South Wales. The price of coal has not fallen, as was to have been expected, for the pitmen have been taught by their leaders that the price may be artificially kept up by shortening the time of labour and restricting the supply. The miner has learned that by working four days in the week he can earn enough wages to supply his wants for the seven, and he does not care to earn more. The idea of "making hay while the sun shines," of laying by money earned by working the ordinary time allotted to mortals for work, is not one by which he is governed,—or, if so, he is prevented from acting upon it by a mighty unseen influence to which he feels bound to render unquestioned submission.

If the colliery proprietors insist on a reduction of wages, or longer hours of work, the result is a "strike." Looked at from a neutral stand-point, it is impossible to conceive a more clumsy device for settling a question between employer and employed, especially in coal-mining. For it is abundantly evident that, in the vast majority of cases, if the proprietors of collieries could see their way to a fair profit, by yielding to the demands of the men, they would do so sooner than expose themselves to the disastrous consequences of a general strike over a large mining district. For let us enquire for a moment what are the consequences to both parties in such a district, for instance, as that of South Wales. To the employer it means loss of customers, cessation of interest on capital invested in the mines, often very large, deterioration of plant and machinery, the mines becoming choked, or becoming filled with water in some instances; and, lastly, the spectacle—which to a man of even ordinary humanity must be hard to endure—of destitution and misery around his own doors, or at least on his own property. To the employed a strike means either a miserable pittance doled out from some Union Fund,—instead of abundant wages,—the exhaustion of the store laid by for "a rainy day," or starvation itself. It means idleness in place of industry, poverty instead of wealth, degradation and demoralisation instead of self-respect. And when all is over, when the war has been waged "to the bitter end," the workman returns to his employment morally and physically impaired; and often, after the loss of a considerable sum in hard cash, commences again with wages no higher than those against which he struck.

Mayhap the result of a strike is to annihilate some branch of manufacture, or to drive it from the district; and the workman finds, when too late, that he has been taking the bread out of the mouth of himself and his family. The ship-building trade of London is a case in point; and in South Wales, where iron-smelting was in some cases a source of little or no profit to the employer, the result of the recent strike has been to close, perhaps permanently, a considerable number of iron-furnaces, whereby a large number of men will lose their daily bread.

If these views were more generally understood amongst the mining population, and if they would exercise that independence of thought and action which is the heritage of every free man, strikes would become a thing of the past; men would work, and the price of each commodity would find its own level according to the laws of political economy. It is to be feared, however, that the mining population is in a state as regards education which is not creditable to a British subject. In some districts, both in Scotland and England, the miners and their families are in a state of gross ignorance, and so wretchedly housed that even decency is out of the question. This may be due, in some measure, to their improvident habits, for the wages they earn are sufficient to provide them with much better accommodation.

But it is also a matter which the employers should look after; and we venture to think that with the inducements offered by a substantial house, with good accommodation for a family, habits of temperance and forethought would be nourished. In some cases suitable residences have been provided, and with good results; and I now have before my mind, in a central county of England, the recollection of a substantial row of collier's houses, each with a little garden in front, and with four—or at least three—rooms for the tenant; and not only have they been let to the colliers at fair rentals, but the manager of the works takes good care that they are properly used, and kept in order by the inmates.

Much remains to be done to improve the condition of the working miner; but while he remains often in a state unworthy of a Christian community, can it be wondered at that he should be a ready instrument in the hands of designing men, and surrender the right of private judgment and individual action to self-appointed leaders as ignorant as himself, and far more selfish?

One of the main causes of the present short supply of coal is the refusal of the miners to work full time. Their fathers were accustomed to work five, or even six, days in the week, but the present generation is content with four or four and a half. In consequence of this the mines are unoccupied during two and a half or three days in the week, less coal is raised, the price is advanced—owing both to the short supply and because the proprietor has to recoup himself for the absence of return on his capital during the idle days.

The absence of a desire to accumulate money, the reverse of which may be regarded as in some measure an evidence of civilisation, so general amongst other classes, is a peculiar feature in the case of the miner. Most of us are willing to do extra work in order to add a few pounds a year to our incomes; but with the pitmen of parts of Lancashire, Staffordshire, and Scotland, the case is otherwise. The old motto, "A fair day's wage for a fair day's work," has given place to a new one, "The least amount of work for the largest amount of pay." Unfortunately the time left at the miner's disposal, owing to the short system, is not generally turned to any very high purpose; so that the high wages and short time is of little benefit to the miner himself, and is most injurious to the community at large.

Until, however, the miners can be induced to work during a reasonable time, and supply us with sufficient coal, it behoves the employers to render themselves as independent as possible of their services. This is only to be done by extensive introduction of mechanical contrivances for doing what cannot otherwise be done by hand. Coal-proprietors and managers have been probably too slow in making use of such inventions, and should not delay in turning to account the present state of the labour market. There are many coal-cutting machines invented, all of more or less use in saving human labour; one of these, worked by compressed air, the patent of Messrs. Baird and Co., of Gartsherrie, is stated to be capable of doing the work of forty men, and requires only four men to manage. Supposing this to be so, and as there are about 360,000 men engaged in coal-mining, it is easy to see that, with the universal use of such machines, 9000, or say 10,000, men would be sufficient to turn out the 112 millions of tons, now sufficient for our wants. Of course, from various circumstances, this could not actually be carried out; but the extensive introduction of such machines, by throwing a large number of unemployed hands on the labour market, would have the effect of bringing down wages within reasonable limits, reducing the price of coal, and making it abundant.

The rise in the price of coal cannot, however, be altogether attributed to the action of those engaged in actual mining either as employers or workmen. While the price in London is 50 shillings a ton, it is stated that at the pit's mouth the price is only 20 shillings, while the railway charge is only 6 or 8 shillings. After making all reasonable allowances for cartage and profit by merchants, it ought not to be much over 32 shillings to the consumer. If this be so, the profit to the middle-men or merchants must be beyond all reason, and the public must have recourse to self-protection. This can be done by co-operation, which has worked so advantageously in other departments of produce. Let companies of private individuals be formed who will enter into agreement to deal directly with the coal-proprietor at the pit's mouth, and supply themselves and the public at moderate rates. The undertaking will be attended with no greater difficulties than those already overcome by co-operative societies. We are glad to see that proposals for such co-operative societies for coal supply have actually been made.

It cannot, however, be said that the public has not in some degree merited the pressure it now feels. The waste of coal in manufacturing and household uses has been beyond bounds, and required some sharp remedy in order to work a cure. The only branch of industry where the consumption of coal has been reduced nearly to a minimum is in iron-smelting, and this only in some special districts, such as those of Middlesborough, North Lancashire, and parts of Scotland. What can be done by improvements in the way of economy is curiously illustrated by the history of iron-smelting. At the Clyde iron-works, in 1796, according to the account of Mr. Mushet, no less than 9½ tons of coal were consumed in producing 1 ton of pig-iron. The quantity of coal now consumed has been reduced to 1 ton 14½ cwts. with the hot blast, or 2 tons 3 qrs. of coke. In the Middlesborough district, where the expenditure of fuel has been reduced to a minimum, the quantity of coke and coal combined amounts to 33 cwts. 1 qr. to the ton of pig-iron. In this district the hot air and gases escaping from the throat of the furnace are used for calcining the ore, heating the blast, and generating the steam for driving the blowing-engine.

In steam navigation a much-needed saving is being rapidly effected by the introduction of double cylinders; the first working at high-pressure, the second using the steam over again in conjunction with a condenser. This system has been in use in France for the last twenty years (as I am assured by Prof. O'Reilly), and is now used in all the ocean steamers of modern construction. The saving of fuel may be taken at not less than 25 per cent, and to this advantage there is to be added the important one of additional storage room for goods.

The greatest amount of waste lies in household consumption. The British public seems inveterately wedded to large blazing fires, so constructed as to send three-fourths of the heat up the chimney. Until we overcome our prejudices in favour of the present form of fire-grate, no large amount of saving can be effected; but, unquestionably, some modification combining the heating surfaces of the stove with the cheerfulness and ventilation which are the chief advantages of the present form of open fire-grate would be the means of effecting a large amount of saving in house-fuel. Mr. R. Hunt—our best authority on this subject—considers that the amount of coal consumed for domestic use may be taken at 1 ton per head of the population, and that about one-third of the whole quantity raised is thus consumed,—that is, about 37,000,000 tons. It is probable that the general substitution of stoves, or of such a combination of a stove and grate as above recommended, would result in saving one-third of the above quantity, or twelve millions of tons—a quantity nearly equal to the total export of coal from British ports.

It cannot be supposed that such a social revolution as the re-construction of our house fire-grates involves will be immediately accomplished; but the foregoing statements will be sufficient to show how much lies within our power, both in the way of increasing the output of coal, with diminished cost at the mines, and of economising the domestic supply without the sacrifice of warmth within doors. To the co-operation of the colliery proprietors on the one hand, and of the public on the other, we must look for an increase of supply and a reduction in the demand; the former by the extensive increase of machinery, where hand-labour is now in use; the latter by the introduction of grates constructed with a view to economy. To manufacturers, who are always alive to the principle that "A penny saved is a penny gained," we may trust to avail themselves of every improvement that offers itself in the direction of economy in fuel.* And with regard to the miner, let us hope that the measures which the Legislature have recently passed for securing to the young a sound education may have the effect of rendering the rising generation more industrious, more thrifty, and more independent of influences from without in those matters of which every man should be the sole judge for himself.

II. RAILWAYS AND THEIR FUTURE DEVELOPMENT.

By J. W. Grover, Memb. Inst. C.E., &c.

IT seems a very hard dispensation, though it is an incontrovertible one, that those who have, perhaps, conferred more benefit upon the country during the present century than any of their contemporaries, should reap so little of the reward themselves. The railway shareholder—I mean the original man who honestly read the prospectus, and believed in its statements, and who backed his belief with his money—was too often a victim to his credulity and enterprising spirit.

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* The statement of the Royal Commissioners on this head is satisfactory. While admitting that coal is still wasted largely in consumption, they add that for some time past, in our manufactures, there have been constant and persevering efforts to economise coal by the application of improved appliances for its consumption. Report, vol. i., p. 93.

Yet, why not? all venturers stand a risk. Certainly, mines are more sporting investments, to say nothing of the wonderful and fearful enterprises in unknown corners of the American continent, into which the British public plunge with a confidence worthy a better cause. To these, at least, there is a hope of some sort, remote though it be; the story may be true—diamonds may be found in ant hills—and a good round bonus be the occasional reward of the speculator. But the unfortunate railway shareholder has no such hope; if, after years of earnest expectation, he reaches the grand consummation, the *summum bonum* of five per cent., he is thankful if not satisfied.

Hence, few will embark in fresh railway enterprises legitimately; and this being the case—as it undoubtedly is—we may conclude the summit has been reached. It is true the rivalries of contending companies will induce them to support branch lines, but in themselves these branches are suckers rather than feeders—justly regarded as necessary evils—to be tolerated only where they cannot be avoided, as Dr. Johnson said of notes in books. It is now just four years ago since the Chairman of the London and Brighton Railway Company told his proprietary, who had subscribed four millions towards the construction of a number of branches, that they might as well have used the bank-notes to light their pipes with; therefore, several important authorised lines for which the land had been actually purchased, and, indeed, the works partially completed, were abandoned, to the chagrin of the districts they were intended to serve.

Various attempts have since been made, both in Sussex and Kent, to revive these defunct undertakings, hitherto without success, and as the system now stands, the failures are likely to be repeated, and even success itself promises a crop of financial burthen and disaster.

There is a want of something different from what has gone before, and several engineering gentlemen of eminence have given us their ideas on the subject; the gauge question has been revived by Mr. Fairlie; Mr. Fell has brought out the central rail invention, and others equally novel and ingenious; wire tramways, as they are called, have been built for the conveyance of minerals, and suspended railways for the conveyance of passengers on the tops of posts have been proposed by one eminent advocate. Yet still no practical progress has been made, and we find ourselves where we were when we began.

Now, it is necessary to begin at the beginning, and to consider the very elementary principles of a railway's existence, to look at the physical and financial questions fairly, and having them before us, to settle what is to be done in the future: for depend upon it, the less we ignore the teachings of the past the better; there is no sound progress apart from experience; hence it is that reforms are seldom introduced from without, although it is the external pressure which causes them.

It will be well to deal with the physical questions first of all, before entering upon the financial. The primary conception of a railway is a perfectly smooth, level, and straight road, upon which friction is reduced to the minimum, so that heavy loads may be propelled with the least possible resistance, and at the highest rate of speed.

The earliest type of locomotive engine was designed to run upon such straight and level roads, and it was supposed for many years that locomotives could not climb hills or be made to go round corners.

The first railway carriages were a simple modification of the stage coaches, names and all. It is interesting to look at the curious three-bodied "*Marquis of Stafford*,"—with yellow pannels and windows, filled with ladies in large coal-scuttle bonnets—as shown in one of Ackermann's early engravings of the Liverpool and Manchester Railway, the only substantial difference being that, inasmuch as the railways of those days were made nearly straight, no arrangement was provided for allowing the axles of the carriage to radiate as they do partially in common road vehicles, but both axles were rigidly fastened so as to be immovable.

Again, as all road vehicles have to turn abrupt corners, their wheels are made to turn independently upon their axles, but so soon as flanges were employed to keep the wheels of the railway carriages between two straight rails, this arrangement was found unnecessary, and to obtain greater strength and security, the wheels were rigidly fastened to the axle, and both were compelled to revolve together.

Now, since the primary conception of the perfectly smooth straight road, a great degeneracy has been of necessity taking place; with greatly increased demands, less capital than ever has been forthcoming; consequently the great cuttings and embankments of early days are being abandoned as precedents, and it becomes necessary that railways should approach more closely to the form of ordinary roads, which follow the surface of the ground only—at small cost.

Hence it follows that the rolling-stock itself must revert more nearly to its original pattern, readopting those contrivances which, under altered circumstances, were discarded.

Still keeping to the most elementary principles, for it is these which are forgotten and misunderstood, and yet they should be engraven on brass and hung up in every railway board room in the world. On a common road, a horse can pull a ton weight in a cart behind him on the level at 4 to 4¼ miles an hour, or, which is the same thing, if a weight of 70 lbs. were hung over a pulley and lowered down a well, he could pull it up at the speed mentioned. It is necessary to be a little explicit, as the remarks in this paper are intended for non-technical readers particularly. Now if two strips of iron called rails are laid upon the aforesaid road, the friction is reduced seven-fold, that is to say, the same horse at the same speed could draw 7 tons, the difference between macadam and iron being as 70 lbs. to 10 lbs. This immense advantage, however, disappears when gradients have to be encountered, because the resistance due to gravity becomes so greatly in excess of the resistance due to friction, and is constant in both cases. For instance, if on a common road, up a slope of one foot in ten, the horse takes 5 cwts. in a cart over the macadam, if rails be laid down up the same hill, he could only increase the burthen behind him by a little more than 1 cwt., or, in all, 6¼ cwts.; hence, in this case, the value of the rails is nearly lost. Hence the small use of tramways where hills occur.

Upon a very good macadamised road the resistance due to friction is usually taken at about *one-thirtieth* of the whole load carried; that is to say, if the vehicle were put upon a road sloping 1 in 30 it would just begin to move of itself. But upon a railway, under the most favourable conditions, the resistance due to friction has been reduced to the *two-hundred-and-eightieth* part of the whole load carried; that is to say, the vehicle will begin to move of itself on a gradient of 1 in 280. In considering the work which a horse can perform on a tramway, it is important to bear in mind the question of speed; for, according to the experiments of Tredgold, he can draw exactly four times as much at two miles an hour as he can at five, and it appears that at three miles an hour he does the greatest amount of actual useful work, whereas at ten miles an hour only one-fourth of his actual power is available, and he cannot exert that for an hour and a half; whereas at two and a-half miles an hour he can continue working for eight hours. Having these data before us, it is easy to compare the values of steam and horse-flesh:—Suppose coals to cost in the midland districts 18s. 8d. a ton only, or one-tenth of a penny per lb., and assuming that an average locomotive engine will not consume more than 5 lbs. of coal in the hour per horse-power, the cost of fuel per horse-power will be a halfpenny per hour. Taking the value of the horse's provender at 1s. 9d. a day only, and supposing he works for six hours, that would cost 3½d. an hour against a halfpenny in the case of steam, or, as 7 to 1 in favour of steam; and this result is obtained on the supposition that the horse travels only at three miles an hour.

Now, to sum up the combined advantages, therefore, of an engine on a level railway against a horse on a level common road at 10 miles an hour, we shall find that the former gives an economy over the latter of nearly 300 to 1; at 5 miles an hour it would stand as 115 to 1; and at 2½ miles an hour as 64 to 1.

Such are the enormous advantages of *steam and rails*, and with them does it not seem astonishing that better financial results have not been obtained? There must be something wrong somewhere. As Artemus Ward says, "Why is this thus, and what is the reason of this thusness?"

Speed is the delinquent, and the cause of the loss of the great primary advantages: the vehicles on railways are propelled very fast; hence they involve great strength in their construction, and enormous weight in proportion to the paying load carried.

An old stage coach, according to Nicholas Wood, weighed only 16 to 18 cwts., and would carry upwards of 2 tons of paying passengers with their luggage, or about $\frac{4}{10}$ths of a hundredweight of dead load to every hundredweight of paying load. Now, a third-class carriage with four compartments would represent 2·8 cwts. of dead weight to every 1 cwt. of paying load. Therefore the stage coach has the advantage over the third-class railway carriage of 6½ to 1.

It becomes impossible to institute any absolute comparison between roads and railways at speeds above 10 miles an hour, because such speeds are impossible on the former for any considerable distance. Again, the question of gradient has to be noticed, for in the preceding remarks a level road and a level railway have only been considered.

As has been explained, where steep gradients occur, the resistance due to gravity so much outweighs that due to friction that rails afford a comparatively insignificant advantage, and one which is entirely lost if the stock has to be increased in weight 6½ times.

It may easily be shown that on a gradient of 1 in 10, for instance, taking the foregoing figures, that the advantages of a steam-worked railway over a horse-worked road would be a little more than one-fourth, if the stock on the former be only 6½ times heavier in proportion than the latter would require. Hence it follows that no railway having gradients of 1 in 10 could be worth making (assuming such to be possible) unless the stock upon it were assimilated to that of the ordinary omnibus or stage coach-type.

In former times calculations were made by Nicholas Wood of the comparative costs of conveyance on ordinary roads by horses; he showed that on an average a stage waggon could carry at the rate of 2½ miles an hour profitably at 8d. a ton per mile; that a light van or cart at 4 miles an hour could take for 1s. a mile a ton of goods. Passengers in stage coaches were charged 3d. a mile each, or 3s. 6d. a ton, at 9 miles an hour. Now let us consider what railways actually do. At the present moment coals are conveyed at 5-8d. per ton per mile, at an average speed of 20 miles an hour; and this low rate actually leaves a profit. Excursion trains take passengers at less than ½d. each per mile, at 20 miles an hour, or at 7d. a ton a mile.

Now, bearing in mind the relative proportions of paying and non-paying loads involved in carrying passengers and coals, a simple calculation will show that a ton of passengers could be carried for something less than 1d. a mile, or $\frac{1}{14}$th part of a penny each. For, although passengers require station accommodation, they unload themselves, which coals do not.

In the autumn of 1869, the "Times" took up the railway problem, and in a series of very able articles endeavoured to show the errors of the present state of things. Although advocated by so powerful a pen, the reforms still remain unaccomplished—indeed, uncommenced. It was then shown that in practice every passenger on a railway involved over 2 tons—of iron and timber--to carry him. Or, according to Mr. Haughton (late of the L. & N. W. Railway), no more than 30 per cent of the load which is hauled by a goods train represents paying weight, the remaining 70 per cent being dead weight. This seems astonishing, truly, but it is nothing to the passenger trains, where only 5 per cent, or even less, of the load pays, the remaining 95 per cent being made up of apparently dead and unprofitable material. It is well to keep this clearly in view. In talking about a passenger, with relation to a railway, one must not picture to oneself a respectable English country gentleman, riding, perhaps, some 14 stone, but some Homeric giant, magnified into prehistoric proportions, weightier than an ordinary Ceylonese elephant, and representing about 20 to 25 full sacks of coals, or 2¼ tons.

Yet for three years and more these "facts" have been made manifest, and nothing whatever has been done; and, as matters stand, no alteration of any appreciable extent is possible, or else it would have been effected long ago. High speeds involve high requirements and great strengths in under-frames, in buffers, couplings, axles, and the entire fabric of the vehicle, besides in the engine, demanding large fire boxes and driving wheels. If trains are to run at 60 miles an hour their construction cannot be materially altered without some change in the general system itself. Thus speaks the oracle:—"The railways of the United Kingdom are conducted by an accomplished, scientific, and highly-skilled body of experts, who know their business, do it, and don't talk about it; and who, moreover, take out of the locomotive all they can, and present it freely and *exuberantly* to those whom it is their interest as well as their pleasure to accommodate—the travelling community."

These remarks are but too true; the travelling community has been well cared for; perhaps the unfortunate shareholders in future undertakings should be accommodated too—by a slice in what is to be so freely and "exuberantly" given away to those who have taken no risk in the venture.

Let us proceed to dissect the existing state of affairs financially, and see where the money goes, and how. Perhaps the last year or two have been exceptional; we will take three years ago. Out of every £100 earned £49 have to be paid away in working expenses, leaving £51 to be divided amongst those who built the line.

How are those £49 spent? The table at top of next page will show generally.

The first three items vary according to the rate of speed employed; they form more than one-half of the whole costs, or 54·54 per cent. A very moderate computation would show that if lower speeds were employed, not only could the stock and engines be reduced in weight, but the wear and tear would be considerably mitigated; the 54·54 per cent would be reduced to somewhere about 36 per cent, or 18 per cent less, increasing the available balance for dividend from 51 to 69 per cent, or from 5 to nearly 7 per cent.

	Per cent of Working Expenses.
1. The maintenance of the way and works costs	18·43
2. The locomotive powers	27·93
3. Repairs and renewals of carriages and waggons	8·18
4. Traffic charges (coaching and merchandise)	28·72
5. Rates and taxes	4·02
6. Government duty	2·47
7. Compensation for personal injury	1·63
8. Ditto loss and damage for goods	0·95
9. Legal and Parliamentary	1·44
10. Miscellaneous expenses	6·23
Total	100·00

To put the case more simply, suppose a train earns on an average 5s. 2d. per mile, the working expenses would be 2s. 6d. a mile, made up in the following way:—

	d.
1. Maintenance of way and works	5·53
2. Locomotive powers	8·38
3. Repairs and renewals of carriages and waggons	2·45
4. Traffic charges (coaching and merchandise)	8·61
5. Rates and taxes	1·20
6. Government duty	0·74
7. Compensation for personal injury	0·49
8. Ditto loss and damage of goods	0·29
9. Legal and Parliamentary	0·43
10. Miscellaneous expenses	1·88
Total working expenses	30*d.*

These figures are the A B C of the railway system in England as it now exists, and supposed to be the most perfect in the world, so far as comfort, speed, and constructive skill is concerned; and the most unsatisfactory as far as commercial result goes, returning on the actual outlay little over 4 per cent. It ends in this, practically, that on the most perfectly smooth surface a train costs 2s. 6d. a mile to run it, to carry an average of 70 passengers, thus showing an average of nearly 6d. a ton a mile. As the system now stands nothing better can be hoped for: competition compels extravagance and destructive speeds; and, furthermore, the travelling public have been so spoilt by the useless waste of space in the three classes, with smoking and non-smoking division, that any attempt at reform would be vigorously and successfully opposed.

The present enormous weight of dead load to paying load in England is to be greatly accounted for by the variety of classes and the fluctuating demands for accommodation; for to each class there must be a large margin of allowance.

We have—

First Class.
Ditto Smoking.
Second Class.
Ditto Smoking.
Third Class.
Break van and engine.

Here we have five different sets of travellers to accommodate; and sometimes, as on market days, there will be three times as many persons of one particular class to accommodate as on others; therefore, practically, on each of the five orders nearly treble the average demand must be provided for. It is all very well for main lines, but on branches something else is requisite. Let there be but two classes—

1. Covered carriages, no smoking.
2. Open side cars, smoking.

And by the use of continuous breaks safety can be increased and a break van dispensed with.

We should here have two classes instead of five, and, therefore, bearing in mind that three times the average number carried has to be allowed for, a proportion of six to fifteen in our favour. It cannot be too often repeated that what exists cannot well be altered; the public have acquired certain rights by mere custom, and they must be maintained; but it is in view of future undertakings only, that the terms of the new contract can be revised, as between the public and the coming shareholder. It is, after all, the public's best interest to do away with that which impedes railway development, for it is the public who reap the advantage.

When a little branch railway has to be constructed, why should the country expect a scale of magnificence in works and stations like that upon the main line from London to Liverpool; why should the undertaking be saddled with bankruptcy from its inception, and what is beneficial in itself be converted into a bye-word and a hissing. The fact is, that the world, not excepting engineers themselves, has been educated up to a certain standard of requirements, and hence it is absolutely hopeless to look for any change in England in "Railways." Like the Circumlocution Office, or a Government department, or one of those old-fashioned blowing engines which I have seen in the iron districts, which does its work, and must not be meddled with, or else it would stop altogether, the "machinery" would get out of order by interference, and once out, it could not be readjusted.

A railway is a railway, and you cannot make anything else of it. A "light railway" is a misnomer—a term which has led to a great deal of confusion and loss of money, although it has received the sanction of the Legislature (31 and 32 Victoriæ)—a "light" railway must be a bad railway; therefore it is as well to descend at once from the lofty eminence, and talk about a tramway, *steam worked*, if you will, but still a "tramway," and not a railway; then at once we begin to approach the region of dividend and commercial prosperity, and the investing public can be once more appealed to with prospect of success, and we work on a different scale and without that majesty of design, which must end in *disaster* and disappointment.

Before approaching the practical part of this paper, and showing what really ought to be, instead of what ought not, I should briefly draw attention to the fact of a "light railway" in this country being almost an impossibility—not physically, but from the surroundings. I speak from experience: a branch railway is projected on the ordinary system, and receives the sanction of Parliament; a great deal of difficulty is found in raising the money, as nobody will subscribe who can help it; a director or country gentleman, who promises a thousand pounds or two, does so simply out of patriotic devotion to his district—for its development—and looks upon the money as a *fonde perdu*, irretrievably gone. The town to be benefited is canvassed by a few enthusiastic agents, who succeed in placing a few hundred shares of £5 each amongst the tradesmen, who give as they would to a charitable association. At last it is found that the whole amount got together is infinitely below what is wanted; indeed, only a fractional part of it.

Then an appeal is made to the Board of Trade, to give permission for a reduction in the style of construction,—light rails, light permanent way, light bridges, light stations, all cheap and bad, and in the end most costly—are sanctioned, and twenty-five per cent is knocked off the required capital. All promises well; inspired with fresh confidence, the directors venture on a start, and something begins to show in the country. Then comes the fatal step, the trunk line, which the branch runs out of, never having had any confidence in the little sucker, and having treated it with contempt, if not with hostility, begins to see an actual move, and therefore undertakes to work the line at 50 per cent, perhaps, of the gross receipts. All goes well now, the 50 per cent agreement is what everybody has been crying out for, and at last have got, but it is a new era of misfortune only—the reign of King Stork over King Log.

The working company, before taking over the new property, instructs its engineer to report upon its condition; he is a gentleman who has been used to the substantial abundance of the past; he does not understand the "light" system; to him a light rail is a bad one. His engines weigh 45 tons with their tenders; and he knows the locomotive superintendent will pick out one of the oldest and worst to work this unfortunate branch, besides a few old coaches unfit for the main line, therefore he cannot accept light bridges. Again, he will find that all the gradients have been made steep, and the curves sharp, to avoid expensive earth works; this in his opinion, and justly, would actually involve a heavier permanent way than he is using on his main line, and so on, till the whole thing has to be re-made; and the working expenses—nominally 50 *per cent*, but, in effect, with all sorts of junction charges and renewal claims, over 65 per cent—entirely swamps the "light system," and its specious and delusive economy.

Wise and able men amongst engineers have seen and felt this, and have freely acknowledged that a branch line must be absolutely something *different* from the parent stem, so that it could not be worked in common with and *into it*. Hence, they have advocated change of *gauge*, apart from its own intrinsic merits, as most completely defining the two systems and preventing their overlapping; it certainly does give to the smaller system an independence and integrity which has great advantages in many ways, but the isolation is too complete in a small country like England, already intersected with lines of a generally standard gauge, except in one or two instances, and these especial. For the undeveloped States of Europe and America, for South America and our Indian Empire, where distances are vast and traffic sparse, a gauge narrower than 4 feet 8½ inches can be used with some advantage and economy; and if the country is at all rough or mountainous, with a mineral traffic, then the necessity for the small gauge is paramount, for it then becomes a question of small gauge against no line at all.

The little Festiniog Railway, in North Wales, has been frequently illustrated in support of the arguments for an extremely narrow line, for though only 2 feet wide between the rails it has paid dividends exceeding 12 per cent—that it has been assumed somewhat hastily that the dividend varies inversely as the gauge, and that by halving the width between the rails the profits can be doubled. The fallacy of this argument is proved at Festiniog itself; for there, even on the face of the same grand mountains, overlooking the same fair valley of the Dwryd river, is another line, not a branch of the first, but rather its continuation to the village of Festiniog, though worked and made by an independent company, which has returned no dividend to its shareholders.

The Festiniog Railway proper has great advantages quite exceptional, and these have been turned to the very best account by the skill and energy of Mr. C. Spooner, C.E., the engineer, who, by adopting the Fairlie Double Bogie Engines, has obtained great power under very adverse circumstances and want of room. Yet it must never be forgotten that the elements of success are manifest. Over one hundred thousand tons of slate annually have to be transported, and all down hill: there is not a fifth of the load to take back in the empties; there is no competition whatever. The toll has been nearly treble, at least over double that charged by any other line for many years; and the line has actual agreements with most of the great quarries by which they would be prevented from any independent action to reduce their freights.

The slates in themselves form a most compact and handy class of goods for carriage. The average speed, moreover, of passenger and goods trains does not exceed 8 to 12 miles an hour. All these circumstances prevent us from taking the Festiniog line as any fair example of a system which would work well elsewhere. To visit it, and to enjoy one of the delightful rides up the mountain side, with the panorama of land and sea around and crags above, and look down on the meadows by Maentwrog spread out as a verdant *parterre*, severed by the silver riband-like stream, is a pleasure to be remembered in a life. There is no such thing to be found elsewhere in the wide world; it is unique, and the enjoyment is accompanied by the exquisite sense of having made some new discovery. Let us for a moment analyse the feeling of having unlearnt the great railway lesson one has been learning all one's life till one visited by chance the vale of Festiniog. For this, and this alone, the journey is worth making, and whoever goes with his eyes open will not return empty; he will feel that his preconceived notions of what was necessary to a railway's existence are torn to shreds and scattered to the four winds. His prejudices of railway education will have been shaken to their foundation, if not uprooted altogether, and he will say with the philosopher of the last century, that "all his knowledge only shows him that he really knows nothing at all."

Hence the great success of this Festiniog Railway as an exemplar. Because it is different it has been taken hastily as perfection, and has been recommended in cases to which it is wholly unsuited. Yet honour to it for its great work. The Russian Empire, the North and Southern Continents of America, and now India itself, have not thought it beneath them to learn from the little Welsh Railway; and it may be truly said that it is the first practical step in the right direction, and has awakened men's minds more than anything else to the necessity for something different, and something better.

It will now be the object of this paper to describe a small and very unpretending "steam tramway," constructed by the Duke of Buckingham for the development of his properties in Buckinghamshire, which in the writer's opinion seems to offer the most universally applicable example of what branch railways must be in the future in England, and perhaps in less developed regions of the world's surface.

This little line was commenced on 8th September, 1870, and the first four miles, from Quainton as far as Wotton, were opened on 4th April, 1871; the greater portion of the remainder was used for mineral and agricultural produce in November, 1871, but the last quarter of a mile up to Brill was not brought into use till April, 1872. The main line is nearly seven miles long, and the gauge the same as that upon the adjoining railways, viz., 4 feet 8½ inches.

The cost of this "steam tramway," including sidings and two goods sheds, was rather less than £1400 a mile without land, which belongs principally to the Duke of Buckingham. The gradients between Quainton and Wotton are favourable, the worst being 1 in 78. But from Wotton to Brill they are comparatively heavy, varying from 1 in 100 to 1 in 51, the total ascent in the last three miles being 130 feet. The line is worked by Messrs. Chaplin and Horne, but the maintenance is undertaken by His Grace the Duke, who executed the work with the assistance of his own engineers, and without a contractor. The expenses of maintenance (and certain other works) is at the rate of

£380 a year; the total working expenses being estimated at £650, including 10 per cent interest on two engines; the earnings being at the rate of about £1350 to £1400 a year, leaving a profit for dividend at the rate of over 7 per cent on the outlay, exclusive of land in the first year, a result probably without parallel in the history of English railways.

This little line traverses the most ordinary agricultural country; there are no great slate quarries or manufacturing establishments to create any exceptional trade. The case is one which affords a striking instance of what can be done by steam and rails in common English country. The traffic consists of coal, road metal, manure from London, and general goods inwards; of hay and straw, grain, timber, bark outwards; of cattle inwards from Herefordshire in spring, and fat cattle to London in the winter.

The coaching traffic consists of passengers and milk, at present carried by a Great Western composite carriage, which has been borrowed, and which weighs 8 tons, a great deal too much for the work it has to do.

The line is worked by one 6-horse Aveling and Porter engine, weighing less than 10 tons, and costing about £400. The engine makes two double trips a day, and a second one is now provided. The former is found sufficient, and very low rates are charged, London manures being brought at 1d. per ton per mile throughout.

It should be observed that these engines have no springs, and consequently travel somewhat roughly. Perhaps too great economy has been sought in them; an expenditure of £600 would have ensured a really efficient machine.

The speed employed varies from four to eight miles an hour, and it was not intended to carry passengers at all in the first instance; but the demand for accommodation in the trains was so great that the passenger carriage had to be borrowed, and the numbers carried were 627 in the first four months of last year.

Unfortunately, no statements of the actual costs and earnings are published beyond April last; but the impulse to trade and agriculture, due to the tramway, is extraordinary; and has exceeded the best expectations. The district served is one by no means densely populated, on the contrary, the whole of the villages, including Brill, do not total up to more than 2000 persons, or less than 300 a mile. Three years ago, the idea of making a branch railway to serve such a district would have been considered insanity; for all over the country branch lines are seen, having actual towns upon them, which nevertheless cannot pay any dividend at all, and are frequently obliged to appeal for refuge to the "Court." Yet here we have an actual proof of the capabilities of iron and steam to serve a district, and not to forget its shareholders; for, even after allowing something for the cost of land and administration, such as might be contingent on an enterprise carried out without the aid of one great proprietor, there is in the Wotton revenue a good balance on the right side.

It is only right, in concluding these remarks on this curious branch, to say that there are no platforms, the rails weigh only 30 lbs. to the yard, and the line is not fenced except in grazing meadows; at each main road crossing there is a siding for trucks; the guard issues tickets whilst travelling in the train, the tickets being torn from a book as in a tramway-carriage; one ordinary train is instanced as a fair average down, it consisted of the engine, a Great Western railway-carriage, five empty coal trucks, and three trucks laden with hay, which altogether weighed about 50 tons.

The staff of servants working the train consists of one engine-driver, one breaksman, and one guard; at Messrs. Chaplin and Horne's offices, at Brill, there is a manager and two clerks. The principal traffic is in coal, of which from 100 tons to 140 tons go up weekly.

How many parts of England, and more in Scotland and Ireland, are languishing for want of such humble but efficient steam tramways; how enormously might the productive powers of the soil be increased by such easy access to and from the railway system; every farmer might have the railway wagons brought to his homestead, giving him cheap lime, coal, and manure, and taking out his hay, straw, and cattle; and furthermore, what a field is here opened out for the investment of capital now seeking employment and only finding it in foreign enterprises. By a little careful selection of the country, by the co-operation of the landowners, and with the aid of an occasional paper mill, quarry, or manufactory, such undertakings might be made to pay large and handsome dividends, very much exceeding those obtained in the Wotton tramway. Their development and their success must depend on the landowners themselves: if they will obstinately persist in making all kinds of monstrous claims for severance and land, no investor could reasonably be asked to embark in the scheme; but if they would content themselves with fair rent charges and agricultural values, their properties might be benefited in a way to yield them handsome returns.

As such tramways must necessarily follow the surface of the ground to a great extent, avoiding heavy earth-works, it is worth while to consider what really are the limits of gradient.

If we take two pieces of clean iron and lay one on the other, and gradually lift one end of the lower one till the superincumbent piece begins to slide, we shall find that this sliding takes place at a slope somewhere between 1 in 4 and 1 in 6; this, therefore, is the ultimate co-efficient of friction, and varies according to the condition of the surfaces in contact of the metal.

Let the uppermost piece of iron be taken to represent the engine, the lower one the rail, the wheels of this engine being locked and prevented from turning, it will just stand at 1 in 4 to 1 in 6; therefore, if the wheels are caused to revolve, it can just climb this gradient under the most favourable circumstances.

But rain, fogs, and sleet prevent this result from being arrived at in practice, and engineers seem to agree that 1 in 10 is the most that can be climbed in all weathers with certainty; therefore, taking this as the *datum*, up half that gradient, or 1 in 20, the engine can take a load behind it equal to its own weight, and up 1 in 30 twice that weight.

Therefore a 10-ton engine can haul 20 tons up 1 in 30, or two loaded wagons, of say 5 tons each, carrying 10 tons of paying load: the non-paying load being 10 tons.

It would appear, therefore, that 1 in 30 is about the steepest incline which should be adopted for any length; this gives a rise of 176 feet in a mile, and practically commands most countries.

Near Aberdare Junction, on the Taff Vale Railway, ordinary locomotives can be seen regularly working up 1 in 18, —which is a practical proof of the foregoing statements,— they take loads behind them of 45 tons.

Between Manchester and Oldham, where the traffic is enormous, the gradients reach as high as 1 in 27—and ordinary locomotives with coupled wheels climb this, with loads behind them of 60 to 80 tons regularly.

These instances merely show extreme cases neither to be copied nor commended, but where occasion requires to be employed sparingly as precedents.

If 1 in 30 be the worst place on the tramroad, a 10-ton engine could nevertheless haul two cars containing over 150 passengers up it; this would be more than would be requisite in an agricultural district.

Having roughly defined the limit of gradient, let us finally consider the curves possible, for after all these are the most important questions of all; we have seen that we can get over hills, but we now require to go round corners, or very sharp elbows, as explained before in this paper; the axles of railway carriages are firmly fastened underneath, so that the vehicle has no tendency to follow the curve or lock as an ordinary four-wheel vehicle upon a common road has; hence engineers seldom adopt curves sharper than 660 feet radius on railways, although there are instances as low as 300 feet: the travelling becomes very bad, and the grinding is fearful. That something better can be done has been demonstrated during the last two years; any one can see, in daily use, at the Fenchurch-street station two railway-carriages of four wheels each, mounted on bogies in such a way as to be able to go round very sharp corners; these vehicles are mounted on Grover's patent under-frames, and the results obtained by them in the duration of their tyres, and consequent absence from friction and grind, have been remarkable.

It is true that the ordinary 8-wheeled double bogie vehicle in use on the American railways will do the same thing, but the enormous length and weight of such cars prevents them from being employed profitably on steam tramways; what is really wanted is a short handy vehicle, capable of being shunted and moved about at the station by a couple of men easily.

With respect to the question of engine there is not so much difficulty; a small traction-engine has its wheels very close together, consequently it will take a sharp curve without difficulty; besides which, Mr. Fairlie has constructed engines on his double bogie system, which have immense power, and are capable of going round curves of 50 feet radius; in mountain districts these engines are most valuable, and enable gradients to be worked easily which would otherwise be almost impracticable.

In South America they are in daily use, taking loads of 120 tons up a gradient of 1 in 25, continuous for 11 miles on the Iquique Line, and also on the Mexican railway.

It appears, therefore, that the necessary mechanical difficulties have been practically surmounted; all is ready to hand, the engineer has it in his power to overcome the obstructions which nature has laid in his way, and those only remain which are due to the prejudice of education and human nature; a great lesson has been learnt,—which must be unlearnt,—but the task is not a difficult one if it be met with the spirit of sincere attention and honest endeavour.

A few words more before concluding on the management of existing railways as they stand. It has been taken for granted that, where high speeds are adopted, no substantial change can take place in the strength of the vehicles or the weights of the engines; but why should not express stock be kept distinct, and a considerable reduction be made in that which is meant for ordinary service? Such an economy is being effected on the South London Railway by the present locomotive superintendent, Mr. Stroudley, who has constructed some light and neat carriages, with central buffers, drawn by small engines weighing only 23 tons; these trains are worthy imitation—they are a step in the right direction, and if more fully adopted would give better dividends and reduced fares. The real fact is, that great obstacles are placed in the way of railway officials, in consequence of the division of their responsibility. The locomotive superintendent thinks little about the permanent way—which is not under him, but adopts great strengths and weights, whereby he increases the "life" of his stock. The engineer who has charge of the permanent way complains, but has no remedy. No real improvement is possible until some ruling mind governs each system, and insists upon comprehensive reforms and the adoption of those inventions which guarantee sure economies.

III. CORAL REEFS AND THE GLACIAL PERIOD.

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THERE is nothing which so much helps forward geological science as the study of our globe as it now is. Every fresh discovery in physical geography helps to explain some hitherto mysterious geological fact. The greater part of the world has yet to be travelled over scientifically, and when this is done, the geological science of that day will probably be as much in advance of our present knowledge as our to-day's science is of that of twenty years back.

Geology made a great advance when Darwin explained the mystery of coral reefs, for by their accurate study, geologists learnt how slowly and gradually large tracts of land were submerged, and in what way great thicknesses of limestone could be formed—not by the preservation so much of actual reefs, as by their disintegration and the widespread deposition of coralline sediment.

Geology made yet another great advance when Agassiz, bringing his knowledge of existing glaciers to bear upon certain phenomena in Scotland and England, showed how certainly our now temperate climate was once an arctic one, and that the diluvial phenomena were for the most part easily explained, on the supposition of the existence of a former glacial period.

Of late, the possibility of determining the time that has passed away since that period of extreme cold has animated the hopes of geologists, and the physicist and astronomer combined have brought their knowledge to bear upon the question. The result, so far, is known to all through the various papers of Mr. Croll. But since truth will stand all shocks, and show itself more truth-like after each attack, it should be the aim of geologists to test all theories put forward to explain series of facts in every possible way; and more especially when those theories are supported by mathematical arguments and reasoning is it incumbent upon the students of nature to see well to the ground-work upon which the mathematician builds his indisputable structure.

It is well known that, according to Mr. Croll's explanation of the cause of the glacial period, those agents producing an extremely cold climate over the greater part of the northern hemisphere would give rise to a proportionally hot one in the southern.

Scientific observation in the southern hemisphere is now bringing before us the fact that, at no very distant period, an extreme glacial climate prevailed there also, and, moreover, that the relics of a former great ice-sheet in the southern hemisphere seem as fresh and of equal value to those of the northern ice-sheet.

Coral reefs are, at the present day, confined within the isotherms of 68°. In proportion as the ice-sheets in either hemisphere are extended into lower latitudes, so must the isotherms of 68° approach the equator, and the coral reef zone become restricted. Let us see what would be the probable result upon the distribution of coral reefs, 1st, of an extended ice-sheet in the northern hemisphere, and, at the same time, an increase of heat in the southern; 2nd, of an extended ice-sheet in the southern hemisphere, and a proportional increase of heat in the northern; 3rd, of greatly extended ice-sheet in both hemispheres at the same period.

1. The study of North American geology shows that, during the glacial period, an ice-sheet completely enveloped

that continent down to the parallel of 39°, while the greater part of Northern Europe was similarly ice-clad.

The present southern limit of perpetually frozen ground in the northern hemisphere is, for a great part of its course, between the parallels of 55° and 60°, though, from the southern point of Greenland to the eastern part of Russia, it runs up to a latitude of 70°. The northern isotherm of 68° roughly corresponds to a latitude of from 30° to 35°, so that now there is an average of 25° of latitude between the southern limit of frozen ground and the northern limit of reef-builders, though, at two points, they approach one another within about 15°. If the former be extended southwards as far as the parallel of 40°, are we justified in concluding that the latter would be thrust southwards in a proportional degree, that is, to a parallel of from 5° to 10°? If so, it is clear that the Equator of Heat, instead of being, for the greater part of its course, north of the equator, as now, might be considerably south of it, even supposing the climate of the southern hemisphere to be no warmer than at present. But if, as we are supposing, the southern hemisphere was under a very hot climate, the equator of heat might be still farther removed from the geographical equator.

Under this condition of things, it would seem certain that there could be but a small range of reefs north of the equator, but that they might extend farther south than at present. Supposing the climate of both hemispheres slowly to approximate to that which now prevails, it is evident that the oldest reefs would be found south of the equator. Now the atolls undoubtedly furnish the evidence of greatest antiquity, since their formation—on Darwin's view of their origin—clearly shows a very gradual sinking of land throughout immense periods of time. Hence we should expect to find the greatest number of atolls south of the equator, and the reefs north of it to belong mostly to the classes of fringing and barrier reefs, which is indeed found to be the case. Moreover, could we hit upon some sure average rate of the growth of reefs, and know the exact relation which such rate of growth bears to the rate of subsidence, or otherwise, of the land, we should have, in these more northern reefs, some indication of the time that has elapsed since the close of the glacial period in the northern hemisphere.

In an article in the "Geological Magazine" for January, 1869, I suggested that the present distribution of coral reefs seemed to show that those south of the equator were of much greater age than those north of it, that the sinking of the supposed old Pacific continent, perhaps, commenced long ere the beginning of the glacial period in the northern hemisphere, and continued uninterruptedly all through that period, the atolls being slowly built up throughout the whole of that time, and that, as the northern climate became finally milder, they were gradually extended further north. Dana estimates the subsidence in the Pacific area as not less than 6000 feet, and taking the rate of subsidence and the upward growth of a reef as 1 ft. per century, this would give a period of 600,000 years for the formation of the Pacific atolls, without allowing for any time of intermittent upward movement, or times during which there might be little or no movement in either direction. The instance of the Florida reefs was also brought forward; here some 10 reefs, one within the other, have been formed, probably since the ice-sheet disappeared, only 9° farther north, and each reef being taken at 70 ft. in thickness, and the rate of growth as above, a period of 70,000 years is given for their formation. Hence, on the whole, the present distribution of coral reefs, especially of atolls—for the most part south of the equator—would seem to favour the idea of a glacial climate having prevailed in the northern hemisphere at a much more recent period than in the southern. But, on the other hand, it may be argued, that atolls, perhaps, do not occur north of the equator in any abundance, because the requisite sinking land was not present, and this argument may hold good to a certain extent, especially as it is the very existence of atoll reefs that marks in great measure the broad land of subsidence in the Pacific.

2. Let us now take the case of an extreme glacial climate in the southern hemisphere, and a proportional increase of heat in the northern.

Agassiz, in the results of his South American Expedition, has just shown that an ice-sheet probably enveloped the southern part of South America, down to the latitude of at least 37°, and even supposing the sheet not to have extended so far as that in the north did, on account of the less amount of continental land round the southern pole, are we not justified in concluding that the southern isotherm of 68° and the equator of heat itself would be shifted considerably north, and the growth of coral reefs rendered as *generally* impossible south of the equator as they probably were north of it during the undoubted glacial period? Supposing no glacial period to have visited the northern hemisphere from the time of extreme southern glaciation until now, we should have expected to find north of the equator coral reefs of great thickness, and atolls in great abundance, *provided* *only* the requisite slow subsidence occurred in the northern equatorial region. And the absence of a great coral reef development, in the shape of atolls, north of the equator, points, therefore, either to the want of the requisite slowly subsiding area, *or* to the advent of a cold period *subsequent* to that occurring over the southern hemisphere, and, therefore, checking the coral growth.

3. What now would probably be the condition of things, supposing the extreme glacial climate occurred simultaneously in both hemispheres? Agassiz says,* "Let me state that I have not noticed anything to confirm the idea that the glaciers of the northern hemisphere have alternated with those of the southern hemisphere in their greatest extension, as is assumed by those who connect with the precession of the equinoxes the difference of temperature required for the change. The abrasions of the rocks seemed to me neither more nor less fresh in one hemisphere than in the other. . . ."

Undoubedly, the extreme glaciation in both hemispheres is the most recent of geological changes; both north and south of the equator it is of younger date than the late Tertiary deposits. Since, however, a Miocene, or Pliocene, fauna and flora may not be of the same age precisely in both hemispheres, time being required for the slow progress of new animals and plants into far latitudes, it follows that the glaciation, though affecting rocks of these ages, and therefore posterior to them, may not be of equal age both in the north and south. Granted, however, that the period of glaciation was approximately the same in both hemispheres, does it not follow that tropical life would be hard put to for a place of abode?

In this case the two isotherms of 68° would be made to approach each other from both hemispheres, and the equatorial belt inhabitable by reef-builders very much narrowed; in fact, it is conceivable that during such a simultaneous maximum of cold in both hemispheres the combined effect on equatorial heat would be such as to squeeze out, as it were, some forms of tropical life, and confine others to very narrow bounds. On the present supposition, therefore, if the climate ameliorated slowly from the time of extreme cold north and south, to the present day, we might expect to find the thickest reefs or the greatest number of atolls close about the equator, always provided that there were areas of subsidence sufficient to allow of the free growth of atolls.

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* See his Report of South American Expedition, as given in "Nature," Aug., 1872, p. 272.

There is another point which might perhaps throw some light upon the question. It is well known that high equatorial lands or corn lands south of the equator are tenanted by north temperate forms of life, left on the mountain ranges in such latitudes as the extreme cold of the glacial period decreased. Darwin says,* "From the presence of temperate forms on the highlands across the whole of equatorial Africa, and along the peninsula of India to Ceylon and the Malay Archipelago, and in a less well-marked manner across the wide expanse of tropical South America, it appears almost certain that at some former period, no doubt during the most severe part of the glacial period, the lowlands of these great continents were everywhere tenanted under the equator by a considerable number of temperate forms. At this period the equatorial climate at the level of the sea was probably about the same with that now experienced at the height of from 5000 to 6000 feet under the same latitudes, or perhaps even rather cooler. During this, the coldest period, the lowlands under the equator must have been clothed with a mingled tropical and temperate vegetation."

Now if both hemispheres were simultaneously visited by an extreme glacial period, we should expect to find about an equal share of northern and southern forms left about the equatorial highlands, unless, indeed, the ice-sheet was much more developed in the one hemisphere than the other, which might arise from larger areas of land on one side of the equator than the other. At all events we should not expect to find a very marked preponderance of forms from one side, especially along those equatorial parts with continental tracts of land both north and south.

Again, if the extreme glacial climate has visited the southern hemisphere at a later date than the northern, we should expect to find a preponderance of south-temperate forms of life on equatorial highlands rather than of north-temperate forms, especially along those parts of the equatorial belt where there was continental land to the south. Lastly, if the glacial period prevailed in the northern hemisphere some time after the last cold era in the southern, the north-temperate forms of life would prevail on equatorial highlands almost to the exclusion of south-temperate—these latter being the relics of a former southern cold. What do we actually find to be the case as regards this distribution of north- and south-temperate forms respectively? I again quote Darwin*—"It is a remarkable fact, strongly insisted on by Hooker in regard to America, and by Alph. de Candolle in regard to Australia, that many more identical or now slightly modified species have migrated from the north to the south than in a reverse direction. We see, however, a few southern forms in the mountains of Borneo and Abyssinia. I suspect that this preponderant migration from the north to the south is due to the greater extent of land in the north, and to the northern forms having existed in their own homes in greater numbers, and having consequently been advanced through natural selection and competition to a higher stage of perfection or dominating power than the southern forms."

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* Origin of Species, p. 455.

The fact is evident, then, that northern forms more encroach upon equatorial and southern regions than southern upon equatorial and northern. Is this due wholly to the greater extent of northern land, as Darwin suggests, or to the southward driving force of a cold period having acted *more recently* than the northward driving force; in other words, does it not point to a glacial period having prevailed in the north more recently than in the south?

From this brief consideration of the probable effects of—(1) latest glaciation in northern hemisphere; (2) latest glaciation in southern hemisphere; (3) simultaneous glaciation in both hemispheres; what are our results? Mainly these, I think:—

1. That an extended ice-sheet in the northern hemisphere would necessarily thrust farther south the equator of heat, and consequently the two isotherms of 68° within which limit the reef-builders occur.

2. That this effect would be increased by the southern hemisphere being unduly hot, just in proportion as the northern was unduly cold.

3. Under these conditions coral reefs would occur in greatest force south of the equator, and, supposing the climate in both hemispheres slowly to approximate to what it is at present, we might now expect to find the oldest—and, therefore, probably the thickest—reefs south of the equator.

4. Could some sure standard of the growth of coral-reefs be established, we might, by comparing the reefs north and south of the equator, form some idea of the shortest period of time which could have elapsed since an extreme glacial climate prevailed in either the one or the other hemisphere.

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* Origin of Species, 5th edition, chap. xi., p. 457.

Example: The reefs of Florida may have taken 70,000 years in formation—therefore the glacial climate, with its ice-sheet extending south into 39° north latitude, cannot have ceased *less than* 70,000 years ago, because these reefs have every appearance of having been uninterruptedly formed, and not of being partly pre-glacial and partly post-glacial.* Again, the South Pacific atolls represent a sinking of 6000 feet; this at 1 foot per century, without allowing for intervals, would give 600,000 years for their formation, which from the very nature of atolls must have been continuous—therefore a glacial climate, with an ice-sheet on the continental lands extending north into 37° south latitude, could not probably have existed in the southern hemisphere within that period.

5. It must, however, be remembered that one of the conditions for the formation of atolls is the existence of an area of slow subsidence, which condition may not have occurred in the northern hemisphere within the 68° isotherm.

6. If an extreme glacial climate occurred *latest* in the southern hemisphere, and coincidently with it an extreme hot climate in the north, we might expect to find the oldest coal-reefs *north* of the equator.

7. An extreme glacial climate prevailing in both hemispheres simultaneously would restrict the coral reefs to very narrow limits on either side of the equator, unless, owing to a less amount of land in the southern hemisphere, the ice-sheet there should not be so continuous and extensive, in which case irregularities in their distribution might occur. On this supposition, the age of the most southerly South Pacific atolls would probably indicate the least time that could have elapsed since the ice-sheet disappeared, since atolls necessitate a continuous act of formation, and they could not be formed partly in pre-glacial and partly in post-glacial times.

8. If the glacial climate prevailed last in the northern hemisphere, we should expect to find north-temperate forms of life more numerous than south-temperate on equatorial highlands; if the southern hemisphere was the most recently glaciated, the south-temperate forms would be more abundant on the equatorial highlands than the north-temperate; if both hemispheres were glaciated at the same time there would be about an equal mingling of north- and south-temperate forms about the equator.

9. The facts of the case are—(1) That the largest

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* I believe I am right in saying that there is no evidence of a long break in their formation.

number of atolls and the thickest reefs occur south of the equator; (2) That the north-temperate forms of life in equatorial regions greatly exceed the south-temperate forms.

10. The general conclusion from these facts seems to be, that the northern hemisphere suffered glaciation at the latest period, and that the southern hemisphere was glaciated at a time perhaps more remote from the period of the northern glaciation than that is from the present.

IV. THE PLANET MARS IN 1873.

By Richard A. Proctor, B.A. (Cambridge),
Honorary Secretary of the Royal Astronomical Society.

THE planet Mars, after being unfavourably placed for observation during two years, has returned to a position where he can be studied advantageously. On April 27th he will be in opposition, and therefore due south, or at his highest above the horizon at midnight. Then, also, he will present his largest apparent disc, or very nearly so (at least so far as the present opposition is concerned). Moreover, as will be seen farther on, there are circumstances which render the study of the planet while in our neighbourhood particularly interesting on this occasion. Therefore the opportunity seems a favourable one for entering into the consideration of the various facts of interest which have been made known respecting Mars. But inasmuch as I have on more than one occasion discussed the principal features of the planet, I shall here restrict myself as far as possible to circumstances presenting some degree of novelty, and devote much of my space to the suggestion of observations which should be made by telescopists during the next two or three months.

It will be known to most of my readers that the planet Mars is the only primary member of the solar system whose condition can be studied under circumstances sufficiently favourable to enable us to arrive at satisfactory conclusions respecting the planet's physical condition. Venus approaches us more nearly, and is more brilliantly illuminated; but when Venus is at her nearest she lies directly towards the sun, and her unillumined side is turned towards us. She is more favourably seen when near her elongations, but is then much farther away than Mars at his nearest, at which time he (unlike Venus) is most favourably situated for observation. Moreover, the great brightness with which Venus is illuminated renders the study of her surface exceedingly difficult. A similar remark applies to Mercury; and it need hardly be said that the proximity of Mercury to the sun presents a yet more serious difficulty to the telescopist, in the fact that Mercury is never far removed from the sun as respects apparent position.

On the other hand, when we pass from Mars to the other superior planets, we find that we must necessarily study the surface even of Jupiter and Saturn under conditions very much less favourable than those which exist in the case of Mars. When at his nearest, that is, when he is in opposition near the perihelion of his orbit, Mars is but about 35 million miles from the earth; and even when he is in opposition near aphelion his distance does not exceed 61 million miles. But Jupiter is never less than 360 millions of miles from the earth, and Saturn never less than 732 millions of miles. So that taking the case of Jupiter at his nearest as compared with Mars in opposition near perihelion, we see that in the first place Jupiter is more than ten times as far away, and the apparent dimensions of equal parts of his surface are therefore reduced more than a hundred times as much, and also Jupiter is very much less brilliantly illuminated by the sun. For the least distance of Jupiter from the sun is 452,692,000 miles, the least distance of Mars 126,318,000 miles, the former distance exceeding the latter about $3\frac{3}{5}$ times; and as illumination varies as the square of the distance, it follows that equal surfaces of Jupiter and Mars (both in perihelion) receive from the sun quantities of light in the proportion of about 13 to 1. Now this consideration is important in comparing the circumstances under which we study Jupiter and Mars. For, although in the case of Venus we have spoken of a degree of brightness which interferes prejudicially with observation, yet in the case of Mars and Jupiter we are not troubled with an excess of light, insomuch that the smaller quantity received from (equal surfaces of) Jupiter introduces a difficulty. When the highest magnifying powers are used, on the best observing nights, there is a want of luminosity in the disc of Jupiter which renders the study of his surface more difficult than it would otherwise be.*

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* The intrinsic brightness of Jupiter is not reduced to the same extent as the quantity of light received by equal portions of his surface (compared with that of Mars). Whether this be owing to the greater reflective power of his surface (that is, of the surface which forms his visible disc), or to some inherent luminosity possessed by the planet, is not as yet determined. *Adhuc sub judice lis est.* But that the peculiarity is very noteworthy will appear from the considerations discussed farther on. In fact, Zöllner estimates the reflective power of the surface of Jupiter at more than twice that of the surface of Mars, or greater in the ratio of 624 to 267. Prof. Bond (the elder), of America, estimated the reflective power of the surface of Jupiter still higher.

It is hardly necessary to inform the reader that the point of chief interest in the study of Mars is the determination of the degree in which he resembles or differs from our earth. And in one respect the discussion of this question is more interesting than it would be in the case of a planet like Venus, which is the equal (or nearly so) of the earth in size. Mars is so much smaller than the earth, that although he belongs to the same family—that is to the inner or so-called terrestrial family of planets—the question might well arise whether he belongs in reality to the same order. He is, in fact, nearer to our moon in volume than to the earth, and comes about midway between the earth and moon as respects mass.* Now we know that the moon is totally unlike the earth in all the circumstances which we associate with the requirements of living creatures; and therefore it might well be believed that Mars is as likely to resemble the moon as the earth in this respect, and is even more likely to occupy a position in the scale of creation utterly unlike that which is occupied by either the moon or the earth. It is the discussion of this question, in the light of the evidence obtained by observation, which renders the study of the planet Mars so full of interest.

I will briefly recapitulate what is known about Mars, noting that the matter is more fully dealt with in my "Other Worlds," and in a paper on Mars in my "Essays on Astronomy." It is necessary for me to allude to these prior discussions of my subject, since otherwise the present paper might seem wanting in completeness. My purpose is to treat as briefly as possible of those matters on which I have already touched, in order that as much as possible of the present essay may deal with new matter.

We know first that the surface of Mars is divided into land and water; and that the continents and oceans of Mars have the shapes depicted in the projections which (for another primary purpose, however) illustrate the present essay. The land has a tint suggesting the idea that the chief constituent of the soil may be a substance resembling our sandstones; though on this point it would be unsafe to speculate too confidently, since the observed colour is probably produced by the blending together of a great number of different tints. That the bluish tracts on Mars are oceanic may be inferred almost with absolute certainty, simply because we know that the atmosphere of Mars is at times loaded with considerable quantities of the vapour of water. This the spectroscope has told us; for it need hardly be remarked that for the lines due to water vapour to be seen at all in the spectrum of the planet, the quantity of aqueous vapour then present in the Martial atmosphere must be very great. I remark in passing, to remove possible misapprehensions on the part of those who are unfamiliar with Dr. Huggins's researches on Mars, that he has shown beyond dispute that the water-lines in the spectrum were not due (at the time of observation) to our own atmosphere. Then this result agrees excellently with the observations which had been made for many years on the white spots near the poles of Mars. Sir W. Herschel had come to the conclusion that these spots are the polar snows of Mars, partly because this conclusion seemed justified by terrestrial analysis, and partly because the white spots waxed and waned in magnitude, in accordance with the theory that they are snowy regions waxing in winter and waning in summer. Snows cannot be produced without large water-covered regions; and the bluish tracts have precisely the appearance which we should expect Martial oceans to present. Then, we have already seen that the spectroscope gives evidence of aqueous vapour in the Martial atmosphere. There is then, therefore, a permanent atmosphere (for no physicist can entertain for an instant the belief that the Martial atmosphere consists solely of aqueous vapour). There must, moreover, be winds, and probably clouds and rain, besides those other meteorological phenomena depending on the increase and diminution of the quantity of aqueous vapour present in the atmosphere. Accordingly, the telescopist finds ample evidence of such phenomena in the appearance of Mars. He finds that known Martial lands and seas are often concealed from view, as if under a layer of clouds; he has been even able to watch the gradual dissipation of such cloud-layers, as if under the rays of the sun as it rose higher in the Martial skies. He sees all round the disc of Mars a whitish light, which can be explained as due to rounded or cumulus clouds in the Martial atmosphere.* He notes the greater distinctness of the hemisphere of Mars, which at the time of observation is passing through its summer season, and readily interprets the indistinctness of the other hemisphere as due to greater prevalence of clouds during the Martial winter. He can even recognise a difference in the colour of the planet as a whole, as though at certain times there were a great increase or diminution of the total quantity of cloud in the Martial air.

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* By this I do not mean that his mass is nearly the arithmetical mean between the mass of the earth and that of the moon, but nearly the geometrical mean. Thus the mass of the earth being taken as unity, that of the moon is 0·0114, and the arithmetical mean of these quantities is 0·5057. Now the mass of Mars is 0·118, or less than a quarter of this arithmetical mean. But the geometrical mean between 1 and 0·0114 is about 0·107, which approaches nearly enough to the value of the mass of Mars to justify the remark in the text. It must not be forgotten, however, that as respects the actual quantity of matter he contains, Mars resembles the moon more nearly than the earth.

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* In my "Essays on Astronomy," I show that this explanation is available; and I add as another explanation, the possibility that the morning and evening

All these circumstances indicate a resemblance rather to our earth than to the moon, where, as we know, there is neither water nor any considerable atmosphere; and when we consider the physical relations involved by the circumstances thus far noted, we find much to suggest the idea that Mars deserves to be regarded as a miniature of our earth. It seems reasonable to infer that since the regions where snow is constantly present, extend on Mars to latitudes resembling those which limit our own regions of perpetual snow, there must be a certain climatic resemblance between the two planets, notwithstanding the fact that Mars receives so much less heat (on mile per mile of surface) than the earth. If we remember that the mean distance of Mars from the sun exceeds that of the earth in the proportion of more than 15 to 10, so that the supply of heat from the sun is less in the proportion of 100 to 225, we cannot but be surprised to find that any resemblance of the sort should exist. And yet, unless we adopt a view presently to be discussed, I apprehend that very little doubt can exist upon the subject. For although, as has been well pointed out by Prof. Tyndall, the presence of snow is an indication of the action of heat, it is manifest that it must indicate also the existence of cold, and that the relative extent of the permanent snow regions of a planet must form

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skies of Mars are ordinarily clouded. But although the second explanation is obviously in accordance with the whiteness near the edge of the disc (since at the parts near the eastern edge day is breaking on Mars, while at the parts near the western edge evening is approaching), the explanation must, nevertheless, be abandoned in presence of the fact that near the terminator of gibbous Mars there is a marked loss of brightness. For here if there were a misty sky on Mars, the whitish light should be seen, and would compensate for the greater obliquity of the sun's rays. As such light is not seen near the terminator, the influence clearly is, that the morning and evening skies of Mars are not specially cloudy, but that the white light seen near the edge of the disc depends (according to the first explanation) on the obliquity with which the line of sight falls on those parts. The illustration in my "Essays" shows how this obliquity would result in causing the whole of the light here received to be that reflected from clouds. I do not think any other explanation is possible; certainly I cannot conceive that any reliance can be placed on the influence of Zöllner, that Mars is covered over with hills having a mean slope of 72°. The great point to be determined, however, is whether the edge of the terminator does or does not show signs of evening or morning mists.

a reliable indication of the general climate of the planet. Indeed, it must be noted that when he pointed out the fact to which I have referred, Prof. Tyndall offered no explanation of it. He simply noted the error of those who would seek to explain the former presence of enormous glaciers solely by the action of cold. "Vast masses of mountain ice indicate infallibly," he said, "the existence of commensurate masses of atmospheric vapour, and a proportionately vast action on the part of the sun. In a distilling apparatus, if you require to augment the quantity distilled, you would not surely attempt to obtain the low temperature necessary to condensation by taking the fire from under your boiler; but this, if I understand them aright, is what has been done by those philosophers who have sought to produce the ancient glaciers by diminishing the sun's heat. It is quite manifest that the thing most needed to improve the glaciers is an *improved condenser;* we cannot afford to lose an iota of solar action; we need, if anything, more vapour, but we need a condenser so powerful that this vapour, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow. The problem, I think, is thus narrowed to the precise issue on which its solution depends." Now, it is important to notice that what is here affirmed of glaciers does not apply with equal force to snow regions at the poles of a planet like the earth or Mars. All the snow which covers these regions must have been formed originally by the action of heat. But a degree of heat, very moderate in amount, would cause the evaporation of sufficient quantities of vapour to produce the snow which covers a widely-extended region. In fact, we know that even in the arctic regions mists and clouds are formed, whence eventually snow is produced, and that these mists and clouds are *not* due in all cases to aqueous vapour which has been formed in warmer latitudes, but are actually produced over ice-covered regions in calm weather; when, therefore, no air is arriving from warmer places. Now, manifestly the snow which covers the polar regions of Mars must either have been formed from vapour raised and condensed in those very regions, or else from vapour raised in lower latitudes and condensed near the poles. In the former case, there must be heat enough in the arctic regions to produce evaporation, and therefore, *à fortiori*, the heat in lower latitudes must in that respect resemble the heat we experience in our temperate zones. In the other case, there must be great processes of evaporation, corresponding to those which take place on our earth; there must be winds carrying the moist air polewards (whence, necessarily, winds blowing towards the equator may be inferred); and there must, in fact, not only be general climatic relations resembling those on our earth, but also similar meteorological phenomena.

It is not so easy as has been sometimes supposed (by myself amongst others) to decide between these two solutions. All that the telescope reveals in Mars has been held to show that the latter solution must be accepted. We actually appear to see the clouds, which are formed in Martial temperate regions, showing that great quantities of aqueous vapour are commonly present in the atmosphere over these regions. We know that more heat than that which would evaporate aqueous vapour near the arctic regions must necessarily be expended on the great oceans of Mars, and that therefore aqueous vapour must be raised into the air over these oceans. And we have seen that spectroscopic analysis confirms this conclusion, or rather establishes it as a demonstrated fact.

But we are thus brought into the presence of somewhat serious difficulties.

In the first place, let us remember that the direct supply of heat from the sun is certainly that which has been mentioned above. In other words, the surface of Mars receives, mile for mile, less than 4-9ths of the heat which our earth receives. This heat may be treasured up (as it were) more completely, or owing to some cause unknown may act more efficiently; but there can be no question that no greater amount of heat is actually received. So that we have this first difficulty to encounter, that regarding Mars as a whole, he seems to be more than twice as well warmed as in the nature of things he would be, supposing the condition of his surface and of his atmosphere resembled what we are acquainted with on earth.

But now as to his atmosphere. Let us suppose that it is constituted like the earth's atmosphere, and let us enquire what must be its density and pressure under such and such conditions. But first it may be asked whether we may not be justified in forming some such opinion as to the quantity of air around Mars which is indicated in Mr. Williams's work "The Fuel of the Sun." Here, as is probably known to many of my readers, the assumption is made that every celestial body has a certain proportion of air around it, a proportion somewhat artificially determined by Mr. Williams, as depending on a numerical relation, the necessity of which is not demonstrated by the evidence. Nevertheless, it seems a reasonable assumption that the larger bodies should have a vaporous envelope of greater extent, whether we regard such envelope as originally a portion of the once wholly vaporous mass of the planet or as partially gathered in by the planet's attraction on vaporous matter in the inter-planetary spaces. And if we assume that the quantity of atmosphere would be proportional to the mass of the planet,—that is to the centre or third power of the planet's radius, multiplied by the number representing the density of the planet,—then since the surface of the planet is proportional to the square of the radius, it would follow that the quantity of air vertically above each square mile of a planet's surface would vary directly as the product of the numbers representing the diameter and the density of the planet. This will be thought as probable a conclusion as Mr. Williams's, and in the present instance it leads to a very similar result. We may adopt it provisionally, in order to see what general results we obtain by following such considerations.

Applying this rule to Mars, whose diameter is about 6-11ths, and density about 3-10ths of the earth's, we obtain for the quantity of air above each square mile of the surface of Mars, the expression $\frac{6}{11} \times \frac{7}{10}$, or $\frac{21}{55}$, where the corresponding expression of the earth is unity,—so that, quite nearly enough for our present purpose, the quantity of air above each square mile of the surface of Mars would be 2-5ths of the quantity above each square mile of the earth's surface. But the pressure and therefore the density of the air at the mean level of a planet depend on the quantity of air above each unit of area, and the attraction of gravity at the planet's surface; for this pressure is solely produced by the weight of the air. Gravity on Mars is represented by 0·387, where terrestrial gravity is unity; and multiplying $\frac{2}{5}$ by 0·387 we obtain 0·1548, which represents (on our assumptions) the pressure of the atmosphere on Mars, when unity represents the atmospheric pressure at our sea-level. Mr. Williams deduces from his assumption a pressure of 0·179. According to one view, the mercurial barometer would stand at about $4\frac{3}{4}$ inches; according to the other, at about $5\frac{1}{2}$ inches on Mars.

Now it is not difficult to perceive that with an atmosphere such as this, and a supply of solar heat equal only to 4-9ths of that which we receive from the sun, Mars might present most of the appearances actually observed. This has been shown (very ably, in my opinion) by Mr. Williams; and although I shall proceed presently to consider certain features suggesting a different theory, I must point out that the balance of evidence appears to me to be decidedly in favour of his theory. Meantime I will follow the line of reasoning pursued by Mr. Williams, noting that much of what he says must be regarded as following obviously from the theory on which it is based.

In the first place, it is clear that with so shallow an atmosphere and so small a direct supply of solar heat, the cold in Mars would be intense. The mean temperature would be below the freezing-point. Nevertheless in the day time, especially in low latitudes, the heating power of the solar rays would be considerable. It would not be so intense as on the summits of our loftier mountains, when a mid-day sun is pouring his rays on the snow-masses there, but would correspond rather to the heat of the sun at about ten or eleven on a summer's morning in Switzerland. It would certainly suffice to melt any surface snows, and also the surface ice of the Martial oceans, which on the theory we are considering must be regarded as frozen throughout their depth.

Now, in considering what would follow as the day proceeded, we find some difficulty in deciding whether there would be an inflow toward the warmed mid-day regions or an air-current flowing outwards (we are speaking now of surface-currents). On earth there is a flow of air towards the region where evaporation is taking place, and it has been urged that this is due to the fact that the aqueous vapour, rising by reason of its relative lightness, causes upward currents in the permanent atmosphere, and that thus an indraught is produced. On the other hand, where evaporation proceeds rapidly, there is a great addition to the atmosphere and consequently an increase of pressure, which would tend to occasion an outflow. In the case we are dealing with, the latter effect might prevail; but in any case it is not perhaps very important to consider the question; because, whether the surface-flow were towards or from the region of evaporation, there would be a flow of moisture-laden air from that region. In one case it would be a surface-flow, in the other it would be an upper-air current; but it is immaterial, so far as our present purpose is concerned, whether the outward flow took place in the upper or lower regions of the air.

Then as the day proceeded, and some considerable time before sunset, "a feathery hoar-frost" would begin to fall. "There would," in fact, "be the same kind of action which Sir J. Herschel has described as necessarily taking place in the moon if any water exists on that satellite, and which he compares to the *cryophorus* experiment. There should, however, be some difference between the case of Mars and the moon. The vacuum of Mars being only comparative, the action would be much slower and less decided than in Sir J. Herschel's supposed case; and the mean temperature of Mars being so much lower, the freezing-point and consequent precipitation of a haze of hoar-frost must commence considerably before reaching the actual boundary between light and darkness; at that angular distance, in fact, from solar verticality, where the cooling influences of the planetary radiation,—aided by those of the remaining ground-ice,—must reduce the surface temperature to the freezing-point." "Thus," proceeds Mr. Williams, "there would be no great well-defined masses of vesicular vapour floating irregularly, like our clouds, in the atmosphere of Mars,—no cumulus, no cumulo-stratus, nor even cirro-cumulus clouds; and, excepting at the borders of the Polar ice, nothing denser than a thin veil of stratus or cirro-stratus cloud, formed of ice-crystals,—the kind of cloud or mist which in our atmosphere makes halo round the moon, and only hides her face sufficiently to exaggerate her beauty, like the gauze 'complexion-veil' of the coquette. The mid-day region, and a certain distance round it, would but rarely be subject to this small degree of obscuration, as the sun's heat there should under ordinary circumstances hold all the vapours it had raised in complete and transparent solution."

It will be gathered, from what has been already stated, that while the results thus indicated accord well with the general features of Mars, they do not agree with the observed appearance of the terminator, when Mars is gibbous. I pause to note this circumstance, because it is manifestly important that observation should be specially directed to the examination of the actual brightness near the terminator of Mars; and it chances that, as will presently be more particularly indicated, the present opposition-period of Mars is particularly well suited for the observation of this feature. But it may be also well to note in this place, that in one circumstance the aspect of Mars corresponds well with Mr. Williams's theory. Mr. Dawes makes the following remarks, in describing the appearances presented by Mars during the opposition of 1865, when the planet was particularly well placed for observation:—"On the whole," he says, "my impression has been that Mars has not usually a very cloudy atmosphere. During the last opposition, the permanence and nearly equable distinctness of the principal features, under similar circumstances, was surprising. On no occasion could I satisfy myself that any part was decidedly less distinct than might be expected from the appearance of the other features then visible. The very white spots noticed on a few occasions, which certainly gave the impression of masses of snow or the reflection from the upper surface of masses of cloud, formed the only decided exception, unless we include the somewhat remarkable fact that the short and rather thick dark line plainly seen near the North Pole on November 14th was invisible on the 12th, when the narrow strait extending from that part of the northern hemisphere towards the south and other objects in the same vicinity were well seen. On November 10th, also, the northern extremity of that narrow strait was invisible, though it might have been expected to be quite as well seen as on the 12th, and even better than on January 22nd. These exceptions to the prevalent clearness of the Martial atmosphere, both relate to regions in high Martial latitudes, and therefore literally tend to 'prove the rule.' "

We come next to the very natural and effective explanation of the Martial snow-caps, in Mr. Williams's theory. We have seen how, under the supposed circumstances, there would be a deposition of hoar-frost continually taking place all round the disc of Mars. Now, "the rotation of the planet will produce," as Mr. Williams points out, "a considerable difference in the results of this deposition. All that falls on the east and west sides of the planet will be thawed and evaporated by the next day's sunshine,* so that the maximum accumulation in these directions can be but one night's deposition; but on the north and the south there will be continual accumulation, which will only be thawed up to a certain latitude by the annual summer presentation of either hemisphere to the sun." The distance between the mean limits of the north and south patches of accumulated hoar-frost may be taken as an approximate measure of the diameter of the circle over which the sun's rays are capable of raising the day-time temperature above the freezing-point (or rather perhaps, of melting quite through the deposited layer of light snow)." Here Mr. Williams notes a consideration which suggests an interesting point for observation. He remarks that the boundary of the region, where the evening deposition of hoar-frost was in progress, should not appear so sharp and well defined as the limit of the morning thaw.

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* The part on the west is actually coming into sunshine, so that "the day's sunshine" would be a more correct expression than "the next day's sunshine," as respects this part of the planet.

We come next to a rather sensational feature of the theory, or rather of the consequences attributed to it: "At the poles," says Mr. Williams, "and for some distance around them, the annual amount of deposition must exceed the annual amount of thawing and evaporation, and therefore a gigantic glacial mountain must there accumulate, with a continual growth and tendency to assume a conical form. As the deposition of ice-crystals would commence before actual sunset, and would probably reach its maximum or even be finished before reaching the boundary line of day and night, in consequence of the thinness of the atmosphere of Mars and the resulting rapidity of radiation, the building up of this polar mountain would be very irregular. In mid-winter, the lower slopes of its sides would receive the greatest accessions. With the advancing line of daylight the elevation of the zone of maximum deposition would increase until it reached the summit. This coincidence of maximum deposition with the summit would occur twice a year, before and after midsummer. During the summer, the only regions receiving any deposition at all would be the summit and its immediate vicinity; while, at the same time, its sides would be rapidly thawing by the powerful action of the continuous sunshine of the long arctic summer day. At this season, the slopes of the arctic mountain would be riven by gigantic ice-floods and water-floods, avalanches, glaciers, and torrents."

While admitting as almost a necessary consequence of the supposed condition of Mars that there would be an accumulation of snow towards the poles of the planet, I must confess I cannot follow Mr. Williams in assuming that the snow-caps can attain a thickness sufficient to increase perceptibly the apparent diameter of the planet. It is true that the telescopist recognises an apparent projection of the polar snows beyond the circular outline of the disc. But irradiation affords so sufficient and satisfactory an explanation of this circumstance as to leave in my opinion little to be desired; whereas the accumulation of snowy masses to a depth of several miles appears difficult to accept, when it is remembered how relatively small must be the quantity of aqueous vapour which could be raised into the tenuous Martial atmosphere. Nevertheless, as Mr. Williams has advocated with some ingenuity the theory not only that such masses exist, but that great glacial catastrophes occur which are recognisable by the terrestrial telescopist, I shall venture to quote some observations by the late Gen. Mitchel (the American observer), which seem to accord singularly well with that rather startling theory.

First, let us examine what in Mr. Williams's opinion would happen and be seen :—" The tendency of the summer growth of the summit and undermining of the sides would be," he remarks, " to bring about periodical catastrophes, by the more or less complete toppling over of the mountain cone in the form of a gigantic avalanche. The occurrence of such a catastrophe would be most sensibly indicated to a terrestrial observer by an irregular and temporary extension of the polar whiteness; where the *debris* of the great avalanche had been hurled beyond the general glacial boundary, and had usurped the region of the summer thaw." The evidence quoted by Mr. Williams himself is an observation made by Prof. Phillips, of Oxford, and two practised observers—Messrs. Luff and Blorridge, working with him. " We noticed," says Prof. Phillips, " a gleaming mass of snow very distinct, so much so, that as happened with the south polar snow of 1862, it seemed to project beyond the circular outline, an optical effect no doubt due to the bright irradiation." On this Mr. Williams remarks that, although Prof. Phillips attributes this appearance to irradiation, it may have been due to the actual heaping of the avalanche material of the overthrown polar ice-cone. But the following observations by Mitchel seem far more strikingly to favour Mr. Williams's bold and ingenious hypothesis :—

" I will here record," says Mitchel, at p. 89 of his " Popular Astronomy," " some singular phenomena connected with the ' snow-zone,' which, so far as I know, have not been noticed elsewhere. On the night of July 12, 1845, the bright polar spot presented an appearance never exhibited at any preceding or succeeding observation. In the very centre of the white surface was a *dark spot*, which retained its position during several hours, and was distinctly seen by two friends who passed the night with me in the observatory. It was much darker and better defined than any spot previously or subsequently observed here; and, indeed, after an examination of more than eighty drawings at previous oppositions, I find no notice of a dark spot ever having been seen in the bright snow-zone. On the following evening no trace of a dark spot was to be seen, and it has never after been visible." This is singularly suggestive of the falling away of a great portion of the snow-cone, followed very soon (as would naturally happen) by the snowing over of the cavity thus formed. The other observation is fully as singular:—"On the evening of August 25, 1845, the snow-zone, which for several weeks had presented a regular outline nearly circular in appearance, was found to be somewhat flattened at the under part, and extended east and west, so as to show a figure like a rectangle with its corners rounded. On the evening of the 30th August, I observed for the first time a small bright spot, nearly or quite round, projecting out of the lower side of the polar spot. In the early part of the evening the small bright spot seemed to be partly buried in the large one. After the lapse of an hour or more my attention was again directed to the planet, when I was astonished to find a manifest change in the position of this small bright spot. It had apparently separated from the large spot, and the edges of the two were now in contact, whereas when first seen they overlapped by an amount quite equal to one-third of the diameter of the small spot. On the following evening I found a recurrence of the same phenomenon" (in other words, the phenomenon was shown to be optical, and depending on the relative positions of two great snow-masses). "In the course of a few days," proceeds Mitchel, "the small spot gradually faded from the sight and was not seen at any subsequent observation."

Certainly these observations accord remarkably well with Mr. Williams's theory respecting the polar snows of Mars. The objections to the theory are found mainly in facts already mentioned. Thus it is difficult to understand how a sufficient quantity of the vapour of water should be present in the Martial atmosphere to produce the dark bands seen by Dr. Huggins, if the atmosphere itself (that is the permanent atmosphere) were so tenuous as the theory implies. It must, however, be noted that the tenuity of the atmosphere would encourage evaporation; in fact, the boiling point at the surface of Mars would be so low as 138° with Mr. Wiliams's assumption as to the atmospheric pressure, and lower still with mine. Nor does so great a difficulty arise as at first sight might be supposed from the fact that large Martial regions have at times seemed to be clouded over, since, in the first place, clouds would not be an unfrequent phenomenon in tenuous atmosphere; and under certain circumstances, as for example great atmospheric disturbances or the effects of such arctic catastrophes as Mr. Williams has described, there might be occasional extensions of dense, though perhaps shallow, cloud-layers, or heavy mists, over wide tracts of the surface of Mars. The one great difficulty which, as it seems to me, would be fatal to Mr. Williams's theory, if demonstrably shown to exist, is the darkening near the terminator of the planet. It is possible, however, that this darkening may be shown to be merely relative. It is to be remembered that, assuming Mr. Williams's theory to be true, the region of evening or morning whitening would be very much less foreshortened at the time of corresponding quadrature than as seen when the disc is full. The obvious consequence of this would be that on the side towards the terminator there would be a much broader whitened border, and not only would the phenomenon be less noticeable on that account (since the narrowness of the white bordering is what renders it so remarkable), but the gradation of light would be much slower. Then, from the obliquity with which the solar rays fall on the parts towards the terminator, there is necessarily (whatever theory we adopt), a real defalcation of light there, and this defalcation may probably be more easily recognisable than the mere excess of light due to the whiteness of this part of the disc. In fact, passing from the centre of the illuminated half of Mars to the terminator, we have first the ruddy or greenish tints of the lands or seas, then a gradually increasing whiteness up to the absolute white of the hoar-frost covered region, then a gradual defalcation of light without change of colour; and the sole question is, Is the latter defalcation likely to be more or less recognisable by the telescopist than the deficiency of light in the middle of the disc on account of the ruddiness or greenness there? It is by no means certain what answer is to be given to this question. The subject has not, indeed, been specially studied by telescopists. When it has been studied with due photometric appliances, and under favourable circumstances (for which the present opposition-period of Mars affords an excellent opportunity) it may be possible to form a decided opinion on the exceedingly interesting and important suggestions made by Mr. Williams.

I shall not make many remarks upon the ordinary theory that the meteorological latitudes of Mars resemble those of our own earth, because this theory has been discussed at considerable length in my works referred to above. But there is one point on which I must make a few remarks. If we remember that the power of an atmosphere to increase the mean temperature depends in the main on its density at the mean level of the planet, we shall see that for Mars to have a climate such as that of our earth, there must be much more air above each square mile of the planet's surface than there is above each square mile of the earth's surface. For the density of the air at the sea-level is proportional to the weight of the air above each unit of surface. For this weight, in the case of Mars, to be the same as in the case of the earth, the quantity of air above each unit of surface must be greater in the proportion of 1000 to 387, that being the proportion in which terrestrial gravity exceeds gravity at the surface of Mars. Taking 18 to 7 as sufficiently near, we have the following consequences if we assume that at the surface of Mars the atmospheric pressure is the same as on the earth. We have in the first place a coating of air, which is greater in quantity, square mile for square mile, than on the earth in the proportion of 18 to 7. But it must also be correspondingly greater in depth, for we know that on the earth the pressure is halved at a height of 3½ miles, in other words that half the atmosphere lies below this height. At seven miles the pressure is reduced to one-fourth—that is, three-fourths of the air lie below this level: and so on. Now, in the case of Mars, the reduction proceeds in the same way, but at different heights. We must increase 3½ in the proportion of 18 to 7 to obtain the height above the mean surface of Mars, at which the atmospheric pressure is reduced one-half. This gives nine miles as the elevation required. At a height of 18 miles, the pressure is reduced to one-fourth; and so on. Now on our assumption as to the actual quantity above each square mile of the surface of Mars, the region above the mean level of the planet to a height of 18 miles is occupied by air, having a mean density as great as that of the air below the height of seven miles from the terrestrial sea-level. Moreover, if we assume a height of 35 miles only as that to which the optically effective atmosphere of the earth extends, we get for the corresponding height in the case of Mars no less than 90 miles. Now, remembering that the diameter of Mars is but about 4400 miles, it seems clear that an atmosphere so deep as this should be telescopically recognisable.

But this is not all: if Mars had an atmosphere no denser at the sea-level than the terrestrial atmosphere, he would not have the same climate as the earth; for as we have seen the solar light and heat at Mars are reduced in the proportion of about 4 to 9 as compared with the solar light and heat at the earth. A very much denser, and therefore a very much deeper, atmosphere than that deduced above would be required to produce a Martial climate resembling our own; and even then, it may be questioned whether with his relatively small ocean surface (here I refer to the actual proportion between the extent of land and water on Mars, and not merely to the extent of water surface in square miles), the atmosphere would be sufficiently vapour-laden to produce the required warmth. For it is to be remembered that dry air is almost perfectly diathermanous, as well for the luminous as for the obscure heat-rays, and that therefore the heat of Mars would be freely radiated away into space, unless the air were freely laden with aqueous vapour. It seems difficult to believe that Mars has an atmosphere so deep and dense as the conditions here considered appear to require.

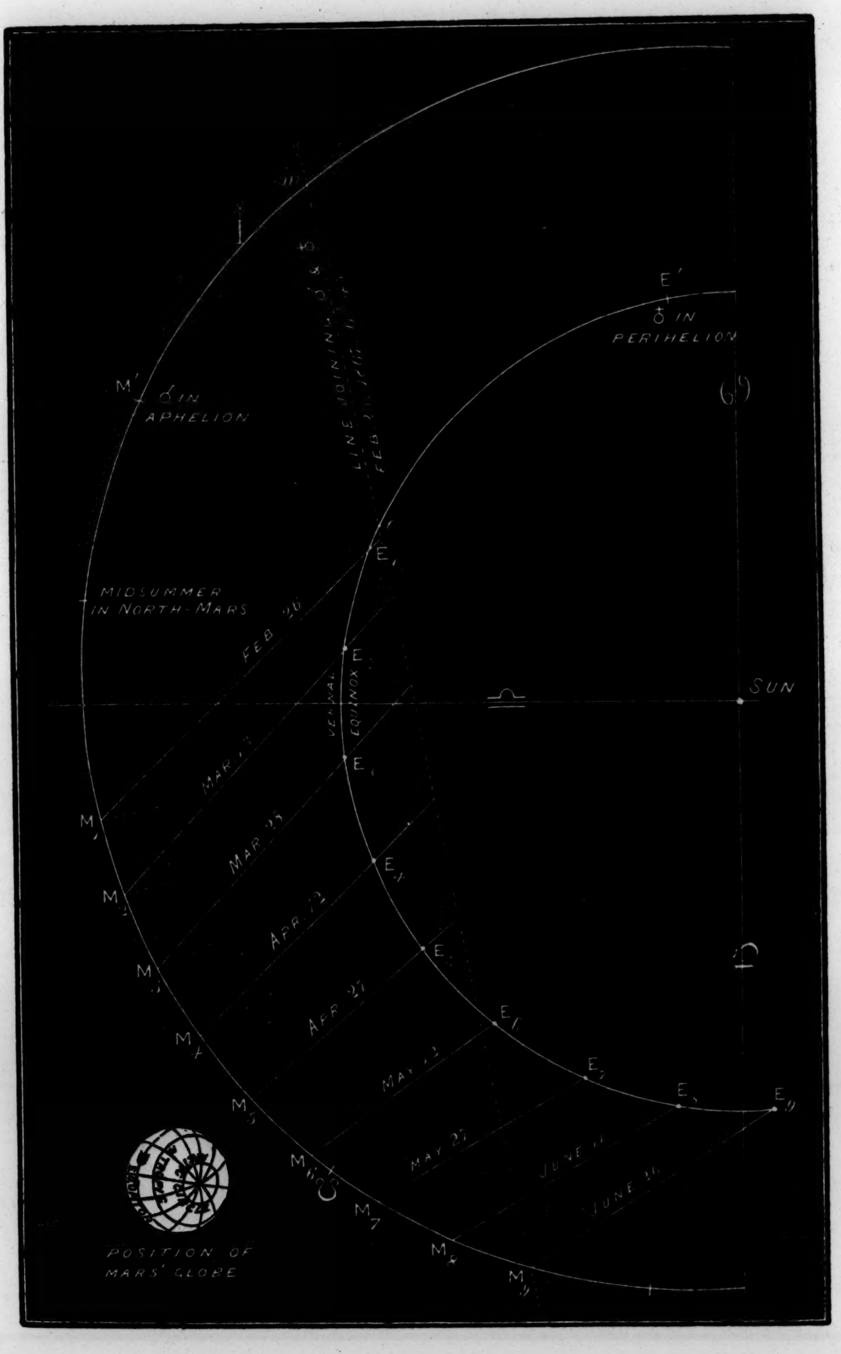
On the whole, I cannot but think that the balance of evidence is in favour of the theory that Mars has a relatively rare atmosphere, and that the various phenomena presented by the planet are to be explained in the way suggested by Mr. Williams.

The reader will perceive that a considerable degree of interest attaches to the study of Mars. We are by no means dealing with a planet whose physical habitudes have been thoroughly mastered and interpreted.

But, apart from these considerations, the present opposition of the planet is one which is peculiarly favourable to the investigation of the planet's condition. A reference to the accompanying figure (and to the plate illustrating this essay) will serve to show this.

In the first place, let it be noticed that when Mars is in opposition, on April 27, he is not far from the place where he is at his mean distance from the sun; for M′ is the place of his aphelion. So far, therefore, as the epoch of opposition is concerned, the present return of the planet may be regarded as having a medium value.

Let it next be noticed that the midsummer of the planet's northern hemisphere occurs when Mars is not far past his aphelion, and that the period illustrated by the figure corresponds to the summer months of North Mars. Now a peculiar interest attaches to this circumstance. Mars resembles the earth (at the present time, and for many years past and to come) in having his solstices near the aphelion and perihelion of his orbit; and the resemblance extends even to the circumstance that the summer of North Mars occurs when the planet is near aphelion, precisely as our summer in the northern hemisphere occurs when the earth is near aphelion. And precisely similar consequences follow from the relation in both cases. Our northern summer is mitigated by increase of distance from the sun, while


LINE JOINING ♂ & ☉
FEB
E'
♂ IN
PERIHELION
♋
M'
♂ IN
APHELION
E1
MIDSUMMER
IN NORTH-MARS
FEB. 26
E2
VERNAL
EQUINOX
♎
SUN
MAR 11
E3
M1
MAR 25
M2
E4
APR 12
M3
E5
APR 27
M4
E6
MAY 12
E7
M5
MAY 27
E8
E9
M6
JUNE 11
JUNE 26
M7
M8
M9
POSITION OF
MARS' GLOBE

the northern winter is mitigated by the reduced distance of the sun; and, on the contrary, the summer heat and winter cold of the southern hemisphere are both intensified. Just so it necessarily happens in the case of Mars, but the effects are more marked, because of the greater eccentricity of the orbit of Mars. The heat received by Mars at mid-summer of his northern hemisphere is less than that received at mid-winter, in the proportion of about 7 to 10. Of course this is more than compensated, in north latitudes resembling our mid-temperate and subarctic zones, by the greater length of the summer's day and the greater height of the midday sun in summer. Nevertheless, the contrast between summer and winter must be most importantly reduced by the relation. On the other hand, the summer of the southern and winter of the southern hemisphere of Mars are intensified by the circumstance that more heat is received directly from the sun at the time of southern mid-summer than at the time of southern mid-winter, in the proportion of 10 to 7. The northern summer is also longer than the southern, to the following extent:—Counting from the vernal to the autumnal equinox the northern summer contains 371½ days, while the southern contains only 296½ days. Thus we have, in the northern hemishere, a long mild summer and a short mild winter; in the southern hemisphere, we have a short but (relatively) hot summer and a long and bitter winter.

It is manifest that, under these circumstances, we may fairly look for a great difference in the aspect of the northern half of the planet during the present opposition period, when the effects of the northern summer (counting still from equinox to equinox) are nearly at a maximum, and that presented during the corresponding opposition-period for the southern half of the planet,—the period, namely, including the opposition of 1864. Then the southern half had passed through its relatively intense summer, and there was a relatively rapid diminution of heat towards the mean heat at the equinox. Now the northern half has passed through its mitigated summer, and a relatively slow diminution of heat is taking place. As several excellent pictures of Mars were taken by Mr. Dawes, in 1864-65,* it will be possible to institute a comparison between the phenomena then observed and those which may be recognised on the present occasion.

It is next to be noticed that the present opposition-period is singularly favourable for observing the gibbous phase of Mars after opposition. For it will be perceived that, even when the line joining the sun and Mars is immediately inclined to the line joining the earth and Mars,—as E_7M_7, E_8M_8, and E_9M_9,—the distance of the planet is not very much greater than when Mars is in opposition at M_5. Thus the disc of the planet, it will be seen (from the illustrative plate), diminishes much more slowly in size after opposition than it had increased before opposition. The telescopist should not lose this excellent opportunity for studying the way in which the disc seems coloured near the terminator. A careful comparison between the part of the disc near the terminator and on the opposite side cannot but prove most instructive. It will be observed that the terminator marks the place where morning is breaking on Mars (before opposition, of course, the terminator marks the place of Martial sunset); accordingly the occasion is favourable for determining whether, supposing there is whitish light near the terminator, that light is sharply defined towards the middle of the disc. Of course this can be done at any epoch of the opposition-period; but it can be best done near quadrature, because either the morning or evening part of the planet is then less foreshortened than at other times.

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* The four best appear among the coloured illustrations of my "Other Worlds."

Next notice another circumstance. Whereas the motion of Mars on his orbit causes the solar elevation north of the Martial equator to continually diminish throughout the period dealt with in the figure,* the elevation of the *earth* north of the Martial equator does not change in the same way. It is easy to see why this is. We may regard Mars, during the opposition-period, with reference to its bearing from the earth when Mars is at M_1, his bearing from the earth is the same as his bearing from the sun when he is near M_6, and accordingly the elevation of the earth north of the equator-plane of Mars is nearly the same on February 26th as the elevation of the sun north of the same plane on May 12th.† Then, as seen from the earth, Mars sways slightly forwards, for E_2M_2 is slightly inclined (and in that sense) to E_1M_1; hence, precisely as happens from the forward motion of Mars round the sun (in this part of his orbit), the elevation of the earth south of his equator-plane slightly diminishes. But from this position right onwards, until the position M_8 (or thereabouts), Mars is swaying backwards (around the earth); hence all this time the elevation of the earth south of the planet's equator-plane is increasing. And lastly, as Mars moves forwards round the earth, after passing M_8, the elevation of the earth south of the equator-plane slightly diminishes. These results are indicated in the table which is given in the foot-note.

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* Precisely in the same way, of course, as in the case of the earth, as specially illustrated in my "Sun-Views of the Earth."

† The table towards the end of the present note gives the actual relations of the Martial globe, as well with reference to the circles and parallels of declination as to the sun. For the convenience of the reader who may care to test the results here tabulated, I give the formulæ and elements from which the table has been calculated.

The elements on which the determination of the axial position of Mars has been based are those given in No. 858 of the "Astronomische Nachrichten," in a paper by Dr. Oudemans upon the observations made by Bessel with the Königsberg heliometer, between the years 1830 and 1837. He gives (as quoted in a note by Mr. Hind in 1867)—

Now it is easy to perceive how these results accord with the presentation of Mars in the nine projections of the illustrative plate. In projection 1 we see how the terminator, continued beyond its northern extremity (at the bottom of the projection), must pass farther from the pole than does

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	°	′	
Longitude of pole of Mars	349	1	for ecliptic.
Latitude	61	9	

Assuming these numbers to apply to 1834·0, we find—

	°	′
Longitude of ascending node of equator of Mars upon his orbit (λ')	80	50·4
Obliquity of martial ecliptic (ι')	27	16·5

And hence—

Ascending node of equator of Mars on the earth's equator (N)	47	33·9
Inclination (I)	39	55·6

For 1873·0 + *t* these values give—

$$N = 47^\circ\ 53' + 0{\cdot}50t$$
$$I = 39\ \ 43 - 0{\cdot}25t.$$

I have adopted these values in the computation of p and l in the accompanying table; p being the apparent inclination of the axis of Mars to the circle of declination, and l the elevation of the earth above the equator of the planet; using the following formulæ:—

Let α be the geocentric right ascension of Mars
δ „ declination „

and Q an auxiliary angle such that—

$$\tan Q = \tan i \,.\, \sin(\alpha - N),$$

then—

$$\tan p = -\frac{\sin Q}{\cos(Q-\delta)} \cot(\alpha - N)$$

and—

$$\tan l = \tan(Q-\delta)\cos p.$$

These formulæ are given by Mr. Hind in the note referred to above, and are the same as are used in the "Nautical Almanac" for determining the position and phase of Saturn's ring. (They are given in full, with others, among the explanations of the tables in my "Treatise on Saturn"). But in Mr. Hind's the actual boundary of the disc. It is clear, moreover, that the upper or southern part of the disc is *viewed* more directly than it is illuminated; for, where the edge of the terminator is there seen, the solar rays are falling tangentially on the globe of Mars, whereas the lines of sight from the earth do not here fall tangentially. (Of course the same remark

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note, by an inadvertency, the denominator in the expression for tan p is written sin $(Q-\delta)$. The following formulæ can be used, if preferred:—

$$\cos l \,.\, \sin p = -\sin I \,.\, \cos(\alpha - N)$$
$$\cos l \,.\, \cos p = \sin I \,.\, \sin(\alpha - N) \sin\delta + \cos I \cos\delta$$
$$\sin l = \sin I \,.\, \sin(\alpha - N) \cos\delta - \cos I \sin\delta.$$

Moreover, if l' be the elevation of the sun above the plane of the ring, λ the heliocentric longitude of Mars, then, with sufficient approximation,—

$$\sin l' = \sin(\lambda - \lambda') \sin I'.$$

Strictly speaking, formulæ corresponding to those given at p. 229 of my "Treatise on Saturn" should be employed, viz., putting—

β = Mars' heliocentric latitude,
ν = longitude of ascending node of Mars' orbit on ecliptic,
and Ω' = arc from ascending node of Mars' orbit on ecliptic to ascending node of Mars' equator on his orbit.

Then assuming—

$$\cos\Psi = \cos(\lambda - \nu) \cos\beta,$$

we have —

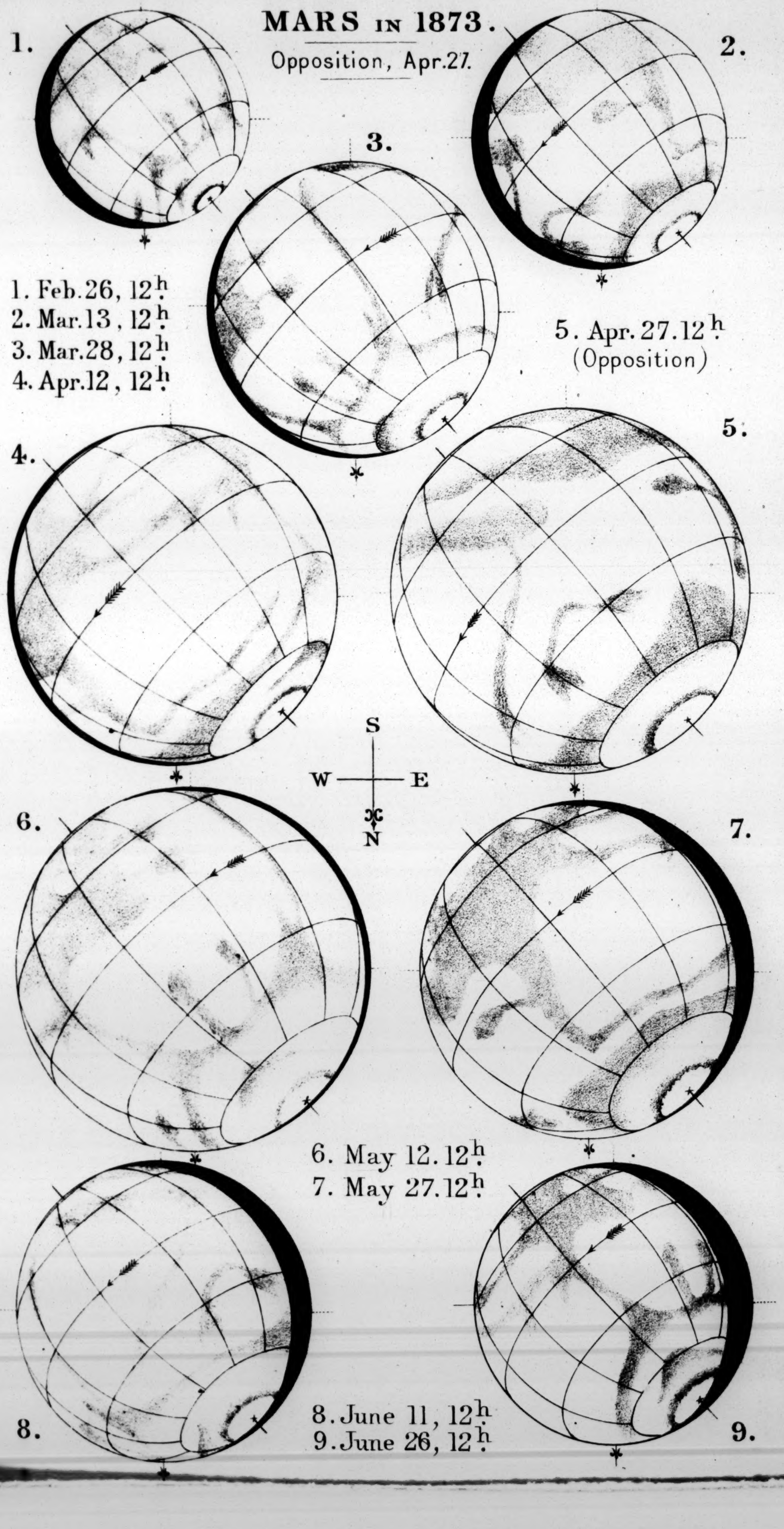
$$\sin l' = \sin(\Psi - \Omega') \sin I'.$$

Date. Greenwich. Noon. 1873.	p.	l.	l'.	$\lambda - \lambda'$.
	° '	° '	° '	° '
Feb. 26	41 4 W.	15 7 N.	25 47 N.	108 21
Mar. 13	41 3	14 7	24 29	115 12
28	41 4	14 15	22 49	122 10
April 12	41 10	15 34	20 47	129 15
27	41 0	17 57	18 23	136 29
May 12	40 31	20 30	15 40	143 53
27	40 2	22 18	12 30	151 27
June 11	40 2	22 59	9 21	159 13
26	40 33	22 36	5 50	167 11

It will be seen that the value of p changes very little during the four months. Usually p changes largely. Thus in the opposition-period of 1866-7, p ranged in value between 9° 50′ and 21° 52′. The reason of the approach to constancy in the value of p during the present opposition is readily seen on a consideration of the figure given above. For we see that, viewed from the earth, Mars first slightly advances, then retrogrades through opposition, and then slightly advances. As this motion takes place along a part of the ecliptic where that circle is descending from the first point of Libra to the tropic of Capricorn, it follows that, so far as this motion is concerned, the apparent slope of the polar axis of Mars to a declination circle (west) at first slightly diminishes, then increases, and towards the end slightly diminishes again. This change depends simply on the inclination of different parts of the ecliptic to declination circles. But the apparent slope of the axis of Mars is also changing precisely as the opening out of his equator is changing (see column under l), being least when the opening out of the equator is greatest, and *vice versa*. So far as this cause of change is concerned, the slope of the axis first slightly increases, then diminishes, and towards the end slightly increases again. Comparing these with the changes due to the other cause, we see that the two changes are compensatory. Hence p remains very nearly constant.

MARS IN 1873.

Opposition, Apr. 27.



1. Feb. 26, 12h.
2. Mar. 13, 12h.
3. Mar. 28, 12h.
4. Apr. 12, 12h.

5. Apr. 27. 12h.
(Opposition)

6. May 12. 12h.
7. May 27. 12h.

8. June 11, 12h.
9. June 26, 12h.

applies to every part of the terminator, but the point has been already considered for the middle parts). Now the result of this is, that the southern parts of Mars, where winter is in progress, are better seen than they would be if the line of sight from the earth were coincident with the line from the sun. Similar remarks apply to projections 2, 3, and 4, but to a gradually diminishing degree.

After opposition the reverse holds,—a fact of more importance, because it is the polar part of the planet which is now more directly viewed than illuminated. It is seen from the projections 6, 7, 8, and 9, how the terminator now passes between the north pole and the northern edge of the disc. It is obvious that the opportunity is thus an excellent one for studying the behaviour of the north polar snow as the summer months pass gradually on towards the autumnal equinox. This opportunity ought not to be lost by those who possess telescopes sufficiently powerful to distinguish the shape and dimensions of the polar snow-caps.

Lastly, it remains that I should make a few remarks on the features of the surface of Mars.

It will be understood that the projections in the illustrative plate are not intended to resemble pictures of Mars.* The land regions and oceans, for instance, are carried right to the very edge of the disc, whereas in reality they are concealed near the edge, under the white light already referred to. These projections are, in fact, masses of Mars, but an orthographic or natural projection, so that they show the various features as they would be seen if Mars were like a terrestrial globe and his aspect not affected by meteorological relations of any sort.

I may be permitted to point out that it was by means of constructions resembling those in the illustrative plate that

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* The woodcut shows the method by which the areographic features of Mars, for the epochs indicated in the plate, have been determined from an observation of Mars made on February 23, 1867, at 6h. 45m. p.m., by Mr. Browning. (The hour in each case is midnight, Greenwich mean time). The picture of Mars then obtained is shown in Plate II. of my "Essays on Astronomy." Between the date of that observation and April 27, 1873, midnight, there is an interval of 194,850,900 seconds. Taking the rotation period of Mars as 88642·73 seconds, I find that the number of rotations of Mars amounts to 2198 + a rotation through 57°. I take the Kaiser Sea as 21° from the central meridian in Mr. Browning's picture (approaching the meridian), and the line joining the Earth and Mars on April 27 makes an angle of about 117° with the corresponding line on February 23, 1867. This obviously amounts to setting Mars 117° *back* in rotation. Thus, instead of 2198 Rot. + 57°, we have 2198 Rot. – 60°, or the Kaiser Sea 81° from the central meridian, instead of 21° as on Feb. 23, 1867, at 6h. 45m. The picture of Mars for April 27, No. 5 of the plate, corresponds with this result. The others have been obtained from similar considerations, account being taken in every case of the changing bearing of Mars from the earth.

I succeeded in interpreting the telescopic pictures of Mars obtained by Dawes, and in forming from them the stereographic and Mercator's charts of Mars which appear in my "Other Worlds" and "Essays on Astronomy" respectively. For every picture which he lent me or had published I constructed the proper orthographic projection, of suitable size, and carefully timed with reference to the actual rotation progress of Mars. It need hardly be said that the results were not found to be in strict accordance, simply because Mr. Dawes in drawing was not able to represent the features of Mars precisely as they were. Eye-judgments must always be, to some slight extent, faulty; and though some of his pictures must have been remarkably accurate (especially those taken in his later years), yet some slight discordances nevertheless existed. The charts, as finally drawn by me, present the features so that their shapes form a sort of mean between the various shapes which result from the separate drawings.

I believe it will be found that the telescopic study of the planet during the approaching opposition, with continual and careful reference to the accompanying projections, will enable any tolerably good draughtsman, possessed of adequate telescopic means, to improve our knowledge of the planet's features. It need hardly be said that, from the various projections given in the plate, the aspect of the planet at any time may be readily determined. The meridians marked on the planet are 30 degrees apart, and, since the planet rotates once upon its axis in 24h. 37m. $22\frac{3}{4}$s., it follows that he rotates so as to carry one of these meridians to the place occupied by the next forwards in a period of 2h. 3m. 7s. very nearly. This would be strictly correct if it were not for the circumstance that, as we see Mars from the earth, account has to be taken of the varying direction in which he is seen. For instance, comparing the position of Mars at M_1 in the figure with his position at M_9, it is manifest that it would be insufficient merely to consider so many rotations and parts of a rotation, in order to deduce his aspect at any given hour when he is near M_9. For the line from M_9 to the earth at E_9 is, as it were, swayed round from the direction occupied by the line from Mars at M_1 to the earth at E_1, and in a direction contrary to that of the planet's rotation; see the globe of the planet in the lower left-hand corner of the picture. And it is plain that this has precisely the same effect as though the planet had rotated so much farther forward.

But although in long intervals this is an important consideration, it is not important in determining the aspect of the planet at any hour on any day intermediate to those corresponding to the projections of the plate. For we see from the figure that the lines E_1M_1, E_2M_2, &c., are in every case inclined at a small angle to their next neighbour lines. Moreover, by determining the aspect of Mars, from the nearest of the projections in point of time, we are sure of not having more than half even of this difference. Also, by means of a protractor, the angular change of the line of sight can be determined from the figure, and taken duly into account.

It is convenient to notice that at any given hour on any night the planet presents appreciably the same aspect as on the preceding night, 37m. 22¾s. earlier.

So far as the *shapes* of the parallels on the different projections are concerned, it is manifest that the change is too slight, from projection to projection, to introduce any difficulty. Nor will any draughtsman find any trouble in reducing or enlarging the scale to the proper dimensions, should he think this necessary. I believe that further explanation of these points is unnecessary, but to prevent any difficulty which may arise I will take an example:—Let us suppose the observer desires to know the aspect of the planet at 1 o'clock on the morning of May 1st (that is, in astronomical time, at 13h. on April 30th). In this case three days and one hour have elapsed after 12h. April 27th, the epoch of projection 5. Now three Martial days are equal to three of our days and 1h. 52m. 8¼s. So that Mars at 1 on May 1st will be rotated as much forward, compared with the aspect observed in projection 5, as corresponds to 52m. 8¼s.; but from meridian to meridian in the projections corresponds to an interval of 2h. 3m. 7s. So that each meridian in projection 5 must be shifted forwards by a less distance than that separating it from the next meridian, in the proportion that 52m. 8¼s. is less than 2h. 3m. 7s., or that 3128¼ is less than 7387. This proportion may be taken as the same as 3 to 7. So that, if we trace the parallels and circular outline of disc from projection 5, and shift each meridian (this also can be done in tracing,—that is, there is no occasion to pencil the meridians as they actually are) forwards by three-sevenths of the space separating it from the next to the left, we have the required meridians and parallels. The features can then be drawn in from projection 5, being carried forward by the same amount as the meridians. In most cases the application of this method requires the features to be completed from one of the other projections. But there is no difficulty in doing this, because the connection between the different projections is very readily traced. Thus, although in comparing 1 and 2 we find nothing to guide us,—for, in fact, the hemispheres shown are almost exactly opposite,—yet projection 3 at once supplies features lying to the right of those shown in 1; and projection 4 at once supplies features lying to the left of those shown in 1. So projections 4 and 5 supply features lying to the right and left of those shown in projection 2. And so throughout the series.

V. THE KENT'S HOLE MACHAIRODUS.

By W. Pengelly, F.R.S., F.G.S.

THE late Rev. John MacEnery, of Torquay, and Kent's Hole, near the same town, rendered each other famous. Those who knew the former tell us that the truth is by no means exceeded in the following eulogy on his gravestone, near the belfry door, in Torre churchyard:—"He had an heart formed for friendship; and, whilst as a clergyman he conciliated all classes by his amiable manners, he inspired respect as a scholar by the vigour of his understanding, his polished taste, and varied learning." Nevertheless, he is now almost exclusively known as the first who made any important discoveries in the great natural mausoleum near which he lived and died.

Though Kent's Hole appears to have been known from time immemorial, and was one of "the lions" of the district in the 18th century, and though fossil bones were discovered in it in 1824, first by Mr. Northmore, and afterwards by Sir Walter C. Trevelyan, it was not until Mr. MacEnery commenced his researches in 1825 that palæontologists and archæologists became aware of its great importance.

Amongst his reputed discoveries none have attracted so much attention as (1st) the inosculation of relics of human industry with bones of extinct mammals, and (2nd) the occurrence of remains of the animal formerly known as *Ursus cultridens*, but now as *Machairodus latidens*; and long after some of the best thinkers had accepted the former they remained sceptical respecting the latter. The difficulty was as follows:—Remains of Machairodus had been found at Epplesheim, in Germany, and in the Val d'Arno, in Italy, but in deposits considerably older than those in Kent's Cavern; moreover, no indication of the genus had been found in any other part of Britain. So strongly was this difficulty felt by one eminent palæontologist that he was wont to express the opinion that MacEnery had obtained some of the specimens found in Italy, that in his collection they had got mixed with the Kent's Hole fossils, and that he had incorrectly, though in perfect good faith, ascribed them to his favourite cavern. Recently, however, all the facts of the case have been collected and published,* and no doubt now remains of the perfect correctness of MacEnery's statements. They have, moreover, been confirmed by the Committee at present exploring the cavern, who had the good fortune to discover there a tooth of the same species.

In this paper, which is to be devoted to the Kent's Hole Machairodus, the following points will be discussed;—

1. The evidence that MacEnery found Machairodus in the cavern.

2nd. The remains of it which he discovered.

3rd. Its era.

4th. Its place in the zoological series.

I. The Evidence that MacEnery found Machairodus in Kent's Cavern.—Mac Enery states that he commenced his systematic "diggings at the close of 1825," † and, as will presently be shown, that he found the fossils in question in January, 1826. The earliest known published mention of the discovery appears in the following notice, of fossils and a communication, received by Professor Jameson from Dr. Buckland, printed in the "Edin. Phil. Journ." for April, 1826.‡ "Professor Buckland has lately sent to Professor Jameson, for the College Museum, several specimens of bone from the hyæna's den at Kent's Hole, near Torquay, all of which he considers as bearing the most decided marks of teeth and gnawing upon them. . . . [In the cavern.] There are also *album græcum*, as at Kirkdale, and stumps of gnawed horns of deer, and the bony bases of horns of rhinoceroses, but no *horns of this animal,* although more than a hundred of its teeth have been already found; also the teeth of many infant elephants, numberless bones of horses, elks, deer, and oxen; and gnawed bones of hyænas, with their single teeth and tusks; also the teeth and tusks of bears, tigers, wolves, and foxes, *and of an unknown carnivorous animal, at least as large as a tiger, the genus of which has not yet been determined.*"*

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* Trans. Devon. Assoc., iii., p. 483, 494, iv., p. 467.

† *Ibid.*, iii., p. 444.

‡ Vol. xiv., p. 363-4.

It is perhaps worthy of note that, as the "Philosophical Journal" was published quarterly, no mention of a discovery made in January, 1826, could have appeared in its pages earlier than in the number for April of the same year—that from which the foregoing quotation has been taken.

If any doubt exists as to the great "unknown carnivorous animal" being Machairodus, it will probably be removed by the following extract from a letter sent by Dr. Buckland to Mr. MacEnery, and of which a copy is preserved in the archives of the Yorkshire Philosophical Society;—

"Lyons, 14th March, 1826.

"My dear Sir,—I should have forwarded the enclosed† from Paris had I not waited to visit a spot in Auvergne, where they have recently discovered a deposit of animals similar to those of Kent's Cave, in a bed of diluvial sand and gravel.

"The resemblance is still more striking from the fact of there being among them the teeth of your *unknown animal*,‡ which turns out to be the *Ursus cultridens* of Cuvier, which had till now been found only in the Val d'Arno. There is a complete skull of this bear in the collection at Florence.

* * * * *

I have sent the gnawed fragments you gave me to Scotland, and trust that ere this opposition in that quarter will have ceased."

It cannot be doubted that the "unknown animal," which turned out to be *Ursus cultridens* mentioned by Buckland in the letter just quoted, was the "unknown carnivorous animal" he spoke of in his communication to Jameson.

It was well known that Mr. MacEnery intended to publish by subscription an account of his researches. A copy of his prospectus, now before us, shows that it was to be illustrated with thirty plates representing the objects in the natural size, and that specimens of the plates had been prepared, and were on view. At his death, however, in 1841, the work had not been published, nor could his manuscript be found, and the plates appear to have been lost sight of. Subsequently, the manuscript was recovered, and seventeen of the lithographed stones were also found. The work was by no means ready for the press, but in 1859 Mr. E. Vivian, of Torquay, published a compilation from it; and through the liberality of Mr. F. Buckland, whose property the stones had become, he was allowed to have some proof impressions taken for its illustration. The plates are distinguished with letters of the alphabet, from B to T inclusive, J and O being missing. The first sixteen contain figures of the remains of animals, and the seventeenth of flint implements. They all state that the specimens represented on them were found in "Kent's Hole, Torquay;" fourteen of them that they were "lithographed from nature by G. Scharf;" one, F, that it was delineated by "Mary Buckland," and lithographed by "G. Scharf;" the remaining two, H and I, are silent on this point; twelve give the information that the specimens were found by Rev. J. McEnery; one, C, by Rev. L. P. Welland, whilst the remaining four give no information on the subject.

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* These italics are not in the original.

† Letter from the Baron Cuvier to Rev. J. MacEnery.

‡ These words are not italicised in the original.

Plate F contains seven figures representing, in the natural size, different aspects of at least three distinct canines, and has the following label:—"Mary Buckland del., G. Scharf, lithog., Nat. size. Teeth of Ursus cultridens. Found in the Cave of Kent's Hole, near Torquay, Devon, by Revd. Mr. McEnery, January, 1826, in diluvial mud, mix'd with teeth and gnaw'd bones of rhinoceros, elephant, horse, ox, elk, and deer, with teeth and bones of hyænas, bears, wolves, foxes, &c." It is the only plate in the series which was drawn by "Mary Buckland," or bears the date on which the specimens were found, or names the animals with whose remains they were mixed. In short, there was a full recognition of the fact that the discovery was regarded as one of importance. It may be, too, that scepticism respecting it was foreseen and provided for, so far as was possible.

The plate, as we have seen, states that the teeth were found in January, 1826; and this harmonises with the facts, that according to the records of the Geological Society of London, one of them was presented to that body by Mrs. Cazalet, February 17th, 1826, and that Sir W. C. Trevelyan, as he has been so good as to inform us, was at Torquay in 1826, about the end of February and beginning of March; that on the last day of the former he spent some hours excavating in the cavern, and that one of the teeth of Machairodus was given to him by Mrs. Cazalet (he thinks), and not by Mr. MacEnery.

In his manuscript, the whole of which was published in 1869, exactly as he left it,* Mr. MacEnery mentioned the discovery of the Machairodus remains no fewer than seven distinct times, and states that he found them in a branch of the cavern known as the Wolf's Cave.*

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* See Trans. Devon Assoc., iii., p. 191-482.

Of the foregoing statements the following is briefly the sum :—Mr. MacEnery commenced his systematic researches at the close of 1825. In January, 1826, he discovered in the Wolf's Cave, teeth of an animal, which he submitted to Dr. Buckland, who, like himself, was ignorant of their true character. Very shortly after their discovery, he gave two of them to Mrs. Cazalet, his friend and co-religionist, † who presented one of them to the Geological Society of London, on the 17th February, 1826, and the other to Sir W. C. Trevelyan about the end of that month or the beginning of the next. Prior to 14th March, Dr. Buckland, describing the contents of the cavern to Professor Jameson, mentioned the occurrence of "an unknown carnivorous animal at least as large as a tiger, the genus of which had not been determined." Subsequently, Dr. Buckland visited Paris, when he submitted the teeth, or more probably casts of them, to the Baron Cuvier, and on March 14th, 1826, when writing to Mr. MacEnery from Lyons, he informed him that his "unknown animal" had turned out to be the *Ursus cultridens;* adding, and this to one with whose palæntological knowledge he was well acquainted, that previously it had been found only in the Val d'Arno.

If the written statements of Mr. MacEnery, Dr. Buckland, and Sir W. C. Trevelyan be insufficient to establish the proposition that Machairodus remains had been found in Kent's Cavern, we may well despair of evidence. Happily, however, the proposition was confirmed on July 29th, 1872, as has been already stated, when the Committee at present charged with the exploration of the cavern by the British Association discovered another tooth of the same species.

II. *The remains of Machairodus which MacEnery discovered in the Cavern.*—Respecting the remains of Machairodus he found in Kent's Hole, MacEnery says "it is scarce, only five teeth having been found." Proceeding to describe one of the teeth, he says, "Its form is semi-lunar, compressed, and tapering to a point like a blade ; and along the course of the enamel, which occupies nearly one-half of its entire length and assumes a fine edge, it is delicately dentated—*vide* plate F, figs. 1, 2, 3, exhibiting different views of the most perfect tooth. The curved fang was snapped off, and the hollow of the tooth disclosed, which, with its unworn point, shows it to have belonged to a young individual. The other teeth represented in the same plate are truncated at their apex, and despoiled of their posterior serrature, while the anterior indenture is entire. . . . The appearance of the blunted apex of the teeth bespeaks the effect of violence. The part is not worn down and polished as is the case with teeth employed in bruising vegetables, but broken sharply off as if from the act of piercing its foe. . . . The enamel is longitudinally situated, and the base of the fang is distinguished by dotted lines in strong relief?

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* *Ibid.*, pp. 240, 243, 294, 368-70, 371-2, 421, and 456-7.

† Mr. MacEnery was the Roman Catholic Priest at Torquay.

"Judging from the wear of the apex and the solidity of the fangs, three of the specimens belonged to adult individuals. They are all gnawed at their base, and the young one cracked across."*

In a subsequent passage he adds:—"In addition to the canines, I have lately discovered in the same bed a small tooth about an inch long. The internal face of the enamel is fringed with a serrated border. This tooth is distinguished further by two tubercles or protuberances at the base of the enamel, from which the serration springs, and describes a pointed arch on the internal surface, *vide* figs. 8, 9.† The body of the tooth in this specimen is not compressed but rounded. Whether this belongs to an inferior species of *U. cultridens*, or is simply the incisor anterior to the canine of *U. cultridens*, I am not able to pronounce with certainty."‡

This latter tooth was subsequently identified, figured, and described by Professor Owen as the right external upper incisor of his *Machairodus latidens*.‖ MacEnery's statement respecting its size must have been based on a rough guess, not on actual measurement, for, instead of being "about an inch long," it measured, according to Professor Owen's figure, 1·97 inches in length, in a straight line, from the vertex of the crown to the base of the fang.

What has become of the incisor appears to be entirely unknown; but the five canines have all been traced. One of them, as we have seen, was presented to the Museum of the Geological Society of London; and Sir W. C. Trevelyan has recently presented his specimen to the Museum of the

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[*] *Ibid.*, pp. 369, 370.

[†] These figures are not known. From the fact that he does not specify the plate in which they occur, it seems probable that they were to be added to plate F, the last he had previously specified, and that in which the canines of Machairodus were represented.

[‡] *Ibid.*, p. 370.

[‖] Hist. Brit. Fossil Mammals, &c., 1846, pp. 177, 182.

Geological Survey in Jermyn Street, London. The remaining three were in MacEnery's collection at his decease, and were disposed of at the sale of his effects. Dr. Battersby, late of Torquay, says in a letter to us on the subject, "There was a card sold at Mr. MacEnery's sale with three teeth (serrated on each side), and marked *Ursus cultridens.* These were purchased jointly by Dr. Phillips and myself. After the sale was over, Mr. König, of the British Museum, came to me and said he had been particularly anxious to have bought them, but had not observed they were on the card until after it was knocked down. Dr. Phillips and I then agreed to give him one for the Museum. Dr. Phillips sent his to the Museum at Oxford. . . . The third I forwarded to Lord Enniskillen, with a number of other teeth, &c., I had purchased for him." . . . Lord Enniskillen subsequently sent his specimen to the Museum of the College of Surgeons, London. It is unnecessary to add that the specimens are carefully preserved in the five museums named above.

As will subsequently be shown, the upper and lower canines of Machairodus are so very dissimilar as to render it quite safe to assert that the Kent's Hole specimens all belonged to the upper series, and thus to render it certain that at least three individuals of *Machairodus latidens* found their way to Britain; and, from what has been stated, that two of them were adults and perhaps aged, whilst the third was young.

The following questions, however, have lately been raised respecting the actual number of teeth found:—

1. Were there not more than five canines?
2. Were there not two incisors?

1. *The Number of the Canines.*—During the progress of his researches, Mr. MacEnery sent specimens of the cavern remains and casts of the rarer fossils to various museums, and amongst others to London, Paris, York, and Bristol. His present to York included "a correct cast of one of the serrated teeth of the *Ursus cultridens* of Cuvier," and was accompanied by a descriptive letter, dated May 3, 1826, enclosing copies of the letters which, as already mentioned, he had received from Cuvier and Buckland. In the Report on MacEnery's present and communication, drawn up by the Rev. W. V. Harcourt, President of the Yorkshire Philosophical Society, and laid before that body, it is stated that "M. Cuvier . . . . found one of the specimens . . . to be the canine tooth of that species of bear which he has named *Ursus cultridens;*" and from this passage it has been

inferred that an actual tooth, and not a cast merely, formed part of the present sent to Paris. There is nothing, however, in either of the letters to justify this inference. On the contrary, MacEnery's list of the specimens he sent to York closes with the remark that "Similar collections to the one now forwarded have been transmitted to Cuvier for the Paris Museum, to Professor Buckland for the London Geological Society, and to Bristol;" thus rendering it at least probable that, as to York, a "correct cast" only was sent to Paris. That casts *were* sent thither is quite certain, for, when visiting the museum, May 2nd, 1872, we made a special and successful search for them; and whilst they were before us, made the following notes:—"In the Palæontological Museum, in the Jardin des Plantes, there are three plaster casts of teeth of Machairodus from Kent's Cavern, two canines, and one incisor. The crown of the first is broken.

"The following three labels accompany them:—

"Label 1. 'Felis cultridens d'Angleterre, Ost. Pl. xvii.'

"Label 2. 'Modèles en Plâtre de 2 canines superieures donnés, par Mr. Mac-Enri.'

"Label 3. 'Modèles en Plâtre d'un Incisive sup. par Mr. Mac-Enri.'"

There was certainly no actual tooth of Machairodus from Kent's Hole in the collection; and when it is remarked that the casts presented in 1826 had been carefully preserved for forty-six years, it may be concluded that less care would not have been bestowed on an original tooth, and that there is nothing to warrant the belief that more than five canines—the number mentioned by MacEnery—were found in the cavern.

2. *The Number of the Incisors.*—We have already seen that according to MacEnery's statement he found one incisor, and that when describing it he referred to figs. 8 and 9, which do not occur in any of his series of plates which have been recovered, but were perhaps intended to be introduced into "Plate F"—his Machairodus plate.

In 1869, several plates were presented to the Torquay Natural History Society by gentlemen who had obtained them from an executor of Mr. MacEnery, whose property they formerly were; many of them were copies of the seventeen already mentioned, but two of them were new ones belonging to the same series—plates O and U. Besides these were some that certainly did not represent Kent's Cavern fossils, and had nothing whatever to do with the series. There was one, however, a drawing in Indian-ink, containing five figures, two of them representing different aspects of a portion of the upper jaw of a horse, whilst the remaining three were those of two incisors of Machairodus, in all respects closely resembling the incisor of *Mach. latidens* from Kent's Cavern, figured, as already stated, by Professor Owen. Besides the figures, there is nothing on the plate but the words "J. Scharf del, 1837."

On the strength of these three figures it has recently been concluded that MacEnery found two incisors in Kent's Hole,* but, in reply, it may be stated that there is nothing to indicate that the plate in question belonged to the cavern series, or represented Kent's Hole fossils; and that, if it did, it could not have been the plate to which he referred, as it contains but five figures, whilst his reference was to "figs. 8 and 9." In short, it seems impossible to deny that the evidence that MacEnery found more than one incisor is certainly very inconclusive.

It is perhaps worthy of remark that Professor Gervais, in his *Zoologie et Palæontologie Françaises* has the following observation under "*Machairodus latidens*":—"Fossil from England in Kent's Cavern. I cite this species among our fossils of France from a single incisor found near Du Puy (Haute Loire) by M. Aymard, in soil probably diluvian, and which he has communicated to me; this tooth quite resembles, by its crenulated edges, that which was discovered in England by Mr. MacEnery, and that of De Blainville and M. Owen."† Is it possible that the figures in the plate under notice are those of the two Machairodus incisors, found one in Kent's Hole, by Mac Enery, the other near Du Puy, by Aymard, and placed side by side for comparison?

III. *The Era of the Kent's Cavern Machairodus.*—It has been already stated that one of the difficulties in the way of the acceptance of MacEnery's reputed discovery, was that the Machairodus remains found in continental Europe belonged to deposits of higher antiquity—those of Epplesheim and Auvergne being miocene, and those of the Val d'Arno pliocene; and though the difficulty was at least partially removed by the fact that the Kent's Hole fossils, though of the same genus, belonged to a distinct species, it was still held to be so remarkable as to require confirmation. At present, however, the chronological chasm has been almost, if not entirely, bridged over by M. Aymard's discovery of a tooth of the same species near du Puy, and the disinterment in Buenos Ayres of an almost complete skeleton of *Mach. neogaeus*, to be described more fully in the sequel, which, according to Dr. H. Burmeister, was the contemporary of the Megatherium and other pleistocene forms.

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* See "The British Pleistocene Mammalia." By W. BOYD DAWKINS, M.A., F.R.S., and W. AYSHFORD SANFORD, F.G.S., Part iv., Pal. Soc., 1872, pages 185—188.

† *Op. cit.*, 2nd edit., 1859, p. 231.

In discussing the question immediately before us, it will be necessary to give a brief description of the successive deposits in Kent's Hole:—First, or uppermost, was a very dark coloured mud, from 3 to 12 inches in depth, and known as the *Black Mould*. Beneath it was a floor of stalagmite, commonly of laminated and granular structure, and termed the *Granular Stalagmite* or *Floor*. Next below was an accumulation of bright red loam, with about 50 per cent of angular fragments of limestone, and designated the *Cave-Earth*. In certain parts of the cavern this rested on a second or lower floor of stalagmite, of highly crystalline texture, in some places upwards of 12 feet thick, and termed the *Crystalline Stalagmite* or *Floor*. Under this lay, so far as is at present known, the lowest and oldest deposit of the cavern, consisting of sub-angular and rounded pieces of dark red grit, embedded in a sandy paste of the same colour; the whole being known as the *Breccia*. Large coherent masses of the breccia, as well as of the granular stalagmite, occurred in various branches of the cavern incorporated in the cave-earth; thus showing that prior to the introduction of the latter they were more important formations than they are at present.

All these deposits contained bones and teeth of animals. In the uppermost, or black mould, they were those of existing species, but in all below it remains of extinct as well as of recent forms presented themselves. In the cave-earth and the granular stalagmite formed on it, but especially the former, the ordinary cave mammals were very abundant; the hyæna being the most prevalent, but followed very closely by the horse and rhinoceros. Remains of megaceros, ox, deer, badger, mammoth, and bear were by no means rare; whilst those of fox, lion, reindeer, and wolf were less prevalent; and those of beaver, glutton, and Machairodus were very scarce. In the lower deposits—the crystalline stalagmite and the breccia—remains of animals were less uniformly distributed. In some places none were met with throughout considerable areas, whilst in others they formed 50 per cent of the entire deposit; but, so far as is at present known, they were exclusively those of bears. Not only were there no bones or teeth of the hyæna, but none of his coprolites, nor were any of the bones broken after his well-known pattern, or scored with his teeth marks.

The bones found in the black mould, or most modern deposit, differed much in specific gravity from those in the lower accumulations, and were generally so light as to float in water. The remains in the cave-earth and breccia had lost their animal matter, and adhered to the tongue when applied to it, so as frequently to support their own weight; but those from the latter were much more mineralised than the specimens found in the cave-earth.

The following general statements may be of service here:—

1. Animal remains were much more abundant in the mechanical deposits than in the stalagmites.

2. The period represented by the *Breccia* and *Crystalline Stalagmite* may, so far as the cavern is concerned, be termed the *Ursine period;* the deposits having yielded remains of bears only.

3. The period of the *Cave-Earth* and *Granular Stalagmite* may be denominated the *Hyæna period;* the hyæna remains being restricted to these deposits.

4. The period of the *Black Mould* may be called the *Ovine period;* remains of the sheep having been found in but not below this accumulation.

5. The bones of each period were distinguishable by their mineral condition; those in the *Black Mould* being much lighter, and those in the *Breccia* being more mineralised, than the remains yielded by the *Cave-Earth.*

Some of the masses of breccia occasionally incorporated in the cave-earth were found to contain bones possessing all the characters of such as were met with in the undisturbed breccia; and a few fossils, easily distinguishable by their mineral condition, had certainly been dislodged from the breccia or older deposit, and re-deposited in the relatively modern cave-earth, without being attended by any discoverable portion of the accumulation in which they had been primarily interred. Hence the question, "Is not this the History of the Kent's Hole Machairodus?" is one which presents itself when considering the era of that species, and which presses for a distinct and definite reply. Indeed, it has recently received a qualified answer in the affirmative,* but which appears to us not to be borne out by the evidence.

The following is the substance of MacEnery's statements having a bearing on this question :—No teeth of Machairodus *were* found in those parts of the cavern in which the deposit yielded remains of bears only ; in other words, in the breccia. This he regarded as a very noteworthy fact, as he supposed the animal to have been a species of bear. They were met with in the branch known as the *Wolf's Cave*, mixed with the teeth and bones of hyænas, and the gnawed bones of rhinoceros, elephant, and the other ordinary cave-earth mammals. Though some of the remains mixed with them bore marks indicative of contusion, they, though "delicately-edged," bore no such indications. The fang of one of the canines had been broken across, and all the others had been gnawed.* Having carefully examined some of the canines, we can confirm the statement that they are gnawed ; and can add that their mineral condition is that of specimens from the *Cave-Earth*, not the *Breccia*.

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* See Brit. Pleist. Mammals, *Pal. Soc.*, Part iv., 1872, p. 191.

Had the teeth in question been derived from the breccia and re-deposited in the cave-earth, it might have been expected that some remains of the same kind would have been met with amongst the immense number of fossils found in the undisturbed original deposit; but instead of this, neither MacEnery nor the British Association Committee, whose uninterrupted and systematic labours have now extended over eight years, met with the least trace of Machairodus in the breccia. Again, the present explorers carefully re-examined all the deposit broken up, but not removed, by MacEnery in the Wolf's Cave, and they excavated there to a depth greater than that to which he restricted himself; but they neither met with any detached bone or tooth having the mineral character indicative of fossils from the breccia, nor any trace of the older deposit, either as incorporated fragments or *in situ*. When to these facts—important, though negative—it is added that the teeth under notice have the mineral condition betokening the cave-earth, and that they have not suffered abrasion or contusion, which it is scarcely possible to suppose they would have escaped had they undergone dislodgment, transportation, and re-deposition, especially when the very delicate serration of their edges is borne in mind, a very strong case seems to be made out in favour of the proposition that *Machairodus latidens* was a member of the *Cave-Earth* fauna. There is, however, another and a most important fact. As already stated, the fangs of the canines are gnawed ; the work, in all probability, of the hyæna—an animal which seems to have been master of the cavern during the cave-earth era, but of which no indications whatever have been found in the breccia.

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* See Trans. Devon. Assoc., Part iii., pp. 240, 243, 294, 370, 371, and 457.

The conclusion to which the foregoing facts concur in pointing, was confirmed by the incisor found, as already stated, by the British Association Committee, July 29th, 1872. It lay in the uppermost foot-level of cave earth, below the granular stalagmite, and below it were teeth of hyæna, horse, and bear; in short, the evidence shows that the Kent's Hole Machairodus belonged to the cave-earth, or hyæna period; and, should any facts hereafter present themselves proving it to have been a member of the fauna of the Breccia, they will in no way disturb this conclusion, but will simply prove that, like the cave bear, *Machairodus latidens* belonged to both eras.

IV. *The Place of Machairodus in the Zoological Series.*—Remains of animals, all now recognised as belonging to the genus Machairodus, have been found in Italy, Germany, various parts of France, England, Brazil, Buenos Ayres, and the Sewalik hills in India, and have been described under the names of *Ursus cultridens*, *U. etruscus et cultridens*, *U. cultridens arvernensis*, *U. cultridens issidorensis*, *U. depranodon*, *Felis cultridens*, *F. cultridens etuariorum*, *F. megantereon*, *F. megantereon et cultridens*, *F. palmidens*, *Machairodus cultridens*, *M. latidens*, *M. palmidens*, *M. neogaeus*, *Megantereon brevidens*, *M. macroscelis*, *Hyæna neogaea*, *Smilodon populator*, *Munifelis bonaërensis*, *Stenodon*, and *Agnotherium*.

Professor Nesti was the first to describe the large falciform canines from the Val d'Arno, and in 1824 he exhibited them to Cuvier, who referred them to the genus *Ursus*, under the name of *Ursus cultridens*. In 1828, M. Bravard found a complete skull in Auvergne, with the falciformal canines *in situ*, and proved that the jaw was like that of the cat's; hence he proposed that the animal should be called *Felis megantereon et cultridens*. In 1833, Dr. Kaup, in his description of the Epplesheim fossils in the Darmstadt collection, pointed out that the compressed canines had neither the longitudinal grooves nor the two ridges which characterise feline canines, that no carnivorous quadruped had the enamelled crown of the canine so long, or its concave edge so serrated, and that in these respects they resembled the teeth of the *Megalosaurus*,—an extinct species of gigantic land-saurian,—and he proposed a new genus, *Machairodus* (sabre-toothed) for the extinct species to which they belonged.

Besides the upper tusks, Kaup was acquainted with those of the under jaw, which are comparatively very small; and, not thinking that they belonged to the same animal, assigned them to another genus, which he named *Agnotherium.*

Dr. Lund, digging in the bone caves of Brazil, found joints of toes and molars which he thought those of a hyæna, and described them under the name of *H. neogaea* in 1839; subsequently, being convinced by the singular tusk that the animal belonged to a distinct genus, he made it known under the name of *Smilodon populator.* His *Smilodon*, however, was the *Machairodus.*

In 1846, Professor Owen, describing the Kent's Hole Machairodus, says, "In this extinct animal, as in the *Machairodus cultridens* of the Val d'Arno, and the *M. megantereon* of Auvergne, the canines curved backwards, in form like a pruning-knife, having the greater part of the compressed crown provided with a double-cutting edge of serrated enamel; that on the concave margin being continued to the base, the convex margin becoming thicker there, like the back of a knife, to give strength. Thus, each movement of the jaw, with a tooth thus formed, combined the power of the knife and saw, whilst the apex, in making the first incision, acted like the two-edged point of a sabre. The backward curvature of the full-grown teeth enabled them to retain, like barbs, the prey whose quivering flesh they penetrated. . . . One of the largest of the canines of the *Machairodus cultridens* from the Val d'Arno measures 8·5 inches in length along the anterior curve, and 1·5 inches in breadth at the base of the crown. The largest of the canines of the *Machairodus* from Kent's Hole measures six inches along the anterior curve, and one inch two lines across the base of the crown; the English specimens are also thinner or more compressed in proportion to their breadth, especially at the anterior part of the crown, which is sharper than in the *M. cultridens.* These differences are so constant and well marked as to establish the specific distinctness of the large British sabre-toothed feline animal; for which, therefore, I propose the name of *Machairodus latidens* [broad-toothed, sabre-toothed.]"*

It is obvious that Professor Owen acquiesced in separating the animals under discussion from the typical *Felidæ*, that he adopted the generic name of Machairodus proposed for them by Professor Kaup, and that he regarded the Kent's Hole form as specifically distinct from that of the Val d'Arno. The last decision was objected to by the late

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* Brit. Foss. Mam., p. 179, 181.

Dr. Falconer, who says, "The length of the Italian tooth is 8·5 in., and the breadth of the crown at the base 1·5 in., while the corresponding measurements of the English specimens are 6 and 1·2 in. The breadth of the English tooth ought to be only 1·06 in., were the proportion to the length the same as in the Italian. Owen says these differences are constant and well marked. But are they sufficient for a distinction of species, or are the materials sufficiently abundant to affirm their constancy? I think not. In my opinion, the English *Machairodus latidens* is probably the same as the Italian *M. cultridens*." *

It has always struck us that in this passage the case is not stated with the author's well-known usual fairness. Professor Owen *named* his species, no doubt, from the greater relative breadth of the crown of the canine, but he *separated* it from the Italian, not on this account only, but also because of the difference in actual dimensions, the greater relative compression of the English specimens, and the sharper anterior edges of their crowns. Be this as it may, Messrs. Boyd Dawkins and Ayshford Sanford, having stated Dr. Falconer's objection, say, "We consider the British *Machærodus latidens*, Owen, to be distinct from the *M. cultridens* of the Continent;"† and they call attention to Professor Gervais's statement that the incisors in the almost entire skull found in Auvergne by M. Bravard, and admitted by all to be M. *cultridens*, are not crenulated as in *M. latidens*.‡

In 1844, Dr. Franz Xavier Muniz found near Lujan, 12 leagues west of Buenos Ayres, the almost complete skeleton of a beast of prey, a contemporary of the Megatherium, Mylodon, Glyptodon, Taxodon, and Mastodon. Finding nothing like it in Cuvier's *Ossem. Foss.*, he described it under the name of *Munifelis bonaërensis*, in the "Gaceta Mercantil" of 9th Oct., 1845.

It proved, however, to be the skeleton of a species of Machairodus, and in October, 1865, Dr. Herman Burmeister, who in 1861 took the management of the State Museum of Buenos Ayres, succeeded in securing the specimen for his museum, through the munificence of Mr. William Wheelwright, contractor of the Argentine Central Railway from Rosario to Cordova. Dr. Burmeister proposes publishing a full description in the "Anales del Mus. publ. de B.A.," but in the meantime he has sent to his friends in Germany a brief notice of the most important parts of the construction. This paper was "specially printed from the treatises of the Natural History Society at Halle," and is accompanied by a figure, from a photograph of the skeleton as it now stands in the Museum, which shows its excellent preservation. We propose incorporating a very condensed summary of Dr. Burmeister's paper, of which, so far as we are aware, no notice has appeared in British journals, for though the skeleton is not that of *Machairodus latidens*, it is beyond all comparison the most perfect specimen of the genus which has been found, and cannot fail to throw considerable light on his British relative.

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* Palæont. Memoirs, 1868, vol. ii., p. 459.

† Brit. Pleist. Mam., Part IV., 1872, p. 187.

‡ Zoologie et Palæontologie Françaises, 2nd. Ed., 1859, p. 231.

The country between the small towns of Lujan and Meroedes forms an oval trough, running from S.W. to N.E., in the midst of which is the little river on which both towns are situated. It is peculiarly rich in well-preserved skeletons of gigantic animals, most of which are on the level of the water, or a little above it.

As the species is the same as that found in Brazil by Dr. Lund, who, apparently not aware of the researches of Dr. Kaup, described it under the name of *Hyæna neogaea*, in 1839, or six years before Dr. Muniz described his specimen as *Munifelis bonaërensis*, Dr. Burmeister has done the former an act of justice by acknowledging the priority of his specific name, and calling the creature *Machærodus neogaeus*.

Everything about the body resembles that of the *Felidæ*, and but for the skull and teeth no one would be able to distinguish it from that genus. Notwithstanding the great size of its tusks, the animal did not reach the size of the existing lion or tiger, and the cave-lion (*Felis spelæa*) was considerably larger.

The following measurements show that relatively to the length of the body, exclusive of the tail, its skull was shorter than that of either the lion or tiger:—

	Skull.	Body.	Ratio of Skull to Body.
Lion	12·5 in.	66 in.	189 : 1000
Tiger . . .	11·6 in.	60 in.	194 : 1000
Mach. neo. . .	13·0 in.	72 in.	186 : 1000

Though, as shown above, the skull is actually longer, it is much smaller than that of the tiger. In the enormous development of the *crista occipitalis* it resembles the hyæna. The face is of great breadth, which is probably due to the astonishing size of the upper tusks, and the long oval form of the relatively small eye orbits.

The under jaw of *Mach. neo.* is considerably smaller than that of the lion, and only a little longer, but at the same time decidedly much narrower than that of the jaguar; but the palate is much broader than that of the lion or tiger, at least in front. The following measurements, in inches, of the length of the under jaw, from the front edge of the tusk to the back part of the edge of the neck, in some of the larger *Felidæ* and *Mach. neo.* may be of interest here:—Cave-lion (*Felis spelæa*), 11·4; Lion (*F. leo*), 9·4; Tiger (*F. tigris*), 8·3; Ounce (*F. onca*), 7·9; and *Mach. neo.*, 8·7. The under jaw of Machairodus is known with certainty by the forestanding edge-comb of the chin on each side, beside which lies the great canine of the upper jaw. It seems to indicate that the point of this tusk could not be hidden under the lips when the mouth is closed, though the upper lip was much broader and more fleshy than that of the existing *Felidæ*.

The Buenos Ayres skull contains three upper and two lower molars on each side. The foremost of the lower series is wanting, and there is no trace of an alveolus. The number and formation of tubercles on them is quite like those of the feline animals.

The following are the dimensions of the upper canine:—Length of the crown 5 inches, of the gum 1 inch, of the root 4·5 inches; total length in a straight line 10·5 inches. The under tusk is surprisingly small in comparison, and scarcely larger than the upper outer incisor. Both the upper and lower canines are devoid of the longitudinal furrows which the tusks of the real *Felidæ* possess—two upon each side of the upper, and one on the outer side of the lower.

The external upper incisors, like the lower canines, are conical, bluntly pointed, slightly bent inwards, and bluntly three-cornered. In the *Felidæ* the outer incisors, especially in the upper jaw, are much larger than the inner ones, which are of equal size; whilst in the lower series a difference of size is perceptible between the inner and the middle ones on each side. In *Machairodus neogaeus* the difference of size between the three on each side, in each jaw, is much more considerable, and the gradual increase from inwards to outwards is not to be mistaken. The teeth of the upper and lower jaws also harmonise more with each other both in form and size—each one of the lower series being a little smaller than the corresponding incisor in the upper. The following measurements of the crowns clearly show the proportions of the several teeth:—

	Upper.	Lower.
Inner incisor	0·63 inch	0·31 inch
Middle do.	0·79 ,,	0·39 ,,
Outer do.	0·98 ,,	0·83 ,,
Canine do.	5·00 ,,	1·02 ,,

In the form of their crowns, the difference between the incisors of the *Felidæ* and *Machairodus* is very decided; for, instead of being chisel-shaped as in the former, every one in the latter is thoroughly conical, extends to a simple rounded point, and is slightly incurved throughout, the point itself standing perpendicularly. Close to the point are two more or less sharp edges, which run along both sides of the crown and get thick and callous below. At the bottom of the crown they turn inwards, get weaker, and approach each other at an angle, which includes a blunt and scarcely perceptible tubercle. These edges have also slight notches corresponding to those of the under tusk.

The conical form of the incisors, as well as the lancet-like upper canines, shows in a high degree the bloodthirsty nature of the Machairodus. Assuming, as very probable, that the objects of his bloodthirstiness were the great *Edentata* of South America—the Megatherium, Scelidotherium, Mylodon, and Glyptodon—it is clear that a sharp long-pointed set of teeth was necessary for killing animals covered with a hard coat of mail, and only a beast of prey like the Machairodus could have been able to kill them. These large animals did not possess the means of active defence. Even the powerful claws of their fore-legs were of no use. For defence, they had only their clumsy figures and coats of mail. The Machairodus, therefore, required the long sharp tusks and pointed incisors to be able to take hold of and kill his prey. The tusk of a tiger or lion could not possibly have penetrated the skin of a Mylodon or Glyptodon. It harmonises well with this description that the South American species of Machairodus possessed such great upper and relatively small lower tusks; as it was only there that the coated gigantic animals existed. In Kaup's species the upper tusks were smaller and the lower ones larger; and *Machairodus latidens*, as represented by Owen, differs still more from *M. neogaeus*. The incisor figured by Owen has a thicker, but not a shorter, crown than that of the same tooth in the Buenos Ayres skeleton. This shows a much less disproportion in *M. latidens* in the extent of the incisors and tusks, and enables us to show this characteristic as a necessary consequence of a difference of construction for their food.

No part of the skeleton of *Machairodus neogaeus* differs so much as the head from the corresponding part in the existing *Felidæ*. The neck has a length of 15·25 inches. The *atlas* is shorter and a little broader, but not stronger, than that of the tiger; and is much inferior to that of *Felis spelæa*. Its form approaches that of the hyæna. The dorsal vertebræ are fourteen in number, the lumbar six, and the pelvic three. The tail is entirely wanting, but there are indications that it was smaller than in the existing large *Felidæ*, and probably not larger than that of the lynx.

The breast-bone and ribs are perfectly like those in the genus *Felis*. The former consists of nine pieces of bone, with a tenth, or terminal one, of cartilage. There are fourteen pairs of ribs, the first being 6·3 inches long, almost everywhere equally broad, and a little compressed; the second is thinner, and the succeeding ones get much thinner upward, thicker below, and teminate in a knob-like swelling. They increase in length to the seventh, which, like the three following it, is 11·4 inches long; after this they decrease to the fourteenth, which, like the first, measures 6·3 inches. How many of them were *false* has not been ascertained.

The bones of the extremities, taken singly, closely resemble those of the *Felidæ*, but when united it is seen that the fore-arm and lower leg are short in proportion to the upper arm and thigh. This will be clearly apparent in the following table, where the lengths are given in inches:—

	Mach. neo.	*Felis spelæa.*	*F. tigris.*	*F. domestica.*
Scapula	12·99	(?)	9·84	3·42
Humerus . . .	14·96	14·96	12·70	4·01
Radius	10·63	13·78	11·02	3·94
Manus	10·63	(?)	11·02	3·54
Metacarp. med. .	3·54	5·39	4·25	1·27
Pelvis	13·78	(?)	12·70	4·33
Sacrum	4·29	5·04	2·76	0·79
Femur	14·96	16·85	14·17	4·72
Tibia	9·84	(?)	12·70	4·72
Calcaneum . . .	4·33	5·33	4·13	1·22
Metatars. med. .	3·94	5·55	4·96	1·97

Whilst the lower bones of the fore-limbs are thus comparatively short, they are much stouter than those of the existing *Felidæ*. The bones of the lion, the most robust of the genus, scarcely reaches them.

It is clear that an animal like Machairodus, possessing such capacities for securing its prey, required very powerful claws; and these excite astonishment by their size and solidity, especially on the inner toes of the fore paws. Even the same bones in *Felis spelœa*, as figured by Schmerling, are only a little longer, whilst the toes are much larger.

The shortening of the fore-limbs is much greater than that of the hinder. From the size of the scapula, the arm and hand of the Machairodus might have been expected to exceed those of the tiger, especially as the upper arm is much longer and thicker; but whilst the living tiger has a shorter shoulder-blade and a shorter upper arm, it has a longer fore-arm and paw, and the bones are much thinner than those of *Machairodus neogaeus;* hence, the strength of the animal is much less. The tiger is quicker and more versatile, but his power of beating down and grasping his prey is certainly less than that of Machairodus was.

Though in the hind limbs the difference is less marked, the lower leg was 5·12 inches shorter than the thigh, whilst in the tiger the difference is but 1·47 inches. The foot, notwithstanding, is almost of equal length in the two animals, and the heel of Machairodus was even longer than that of the tiger, thus proving the greater power of the former in its hinder extremities also.

There is no further difference between the number, position, and size of the hand and ankles of Machairodus and those of the living *Felidæ* than that all the small bones of the former are much stronger. It is the same with the bones of the toes, which, and especially those of the thumb of the fore-paw, are of extraordinary solidity and size.*

Palæontologists are at present acquainted with the following species of the genus *Machairodus*:—

M. cultridens, found in Italy, Germany, and France.
M. latidens, ,, ,, England and France.
M. palmidens,† ,, ,, France.
M. sivalensis, ,, ,, India.
M. neogaeus, ,, ,, Brazil and Buenos Ayres.

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* See Bericht über ein Skelet von Machœrodus, im Staats-Museum, zu Buenos Aires, von Dr. HERM. BURMEISTER. Halle: Druck und Verlag von H. W. Schmidt. 1867.

† See GERVAIS, *op. cit.*, p. 231.

VI. ATMOSPHERIC LIFE GERMS.

LORD Bacon in the "Novum Organum" (Book II., Aphorism 13), says, "All putrefaction exhibits some slight degree of heat, though not enough to be perceptible to the touch: for neither the substances which by putrefaction are converted into animalculæ, as flesh and cheese, nor rotten wood which shines in the dark, are warm to the touch." He thus gives as a definition of spontaneous generation the conversion of substances, such as flesh and cheese, into animalculæ. The joke of Dr. Johnson on Tom Davies, a bankrupt bookseller, who took to authorship, that he was "an author generated by the corruption of a bookseller," is evidently a hint as to his connection with Grub Street through an illusion to the popular belief.

The first recorded facts undermining the old belief in "spontaneous generation," were those of Redi, published in 1638, leading to the first exact experiments in closed vessels of Needham in 1745, and of Spallanzani in 1765; the experiments with air purified by heating of Schwann, and with air passed through oil of vitriol of Schultze in 1837; the proof that the solid particles of yeast alone can cause fermentation by Helmholtz in 1844; Schrœder and Dusch's experiments with air filtered through cotton-wool in 1854; and the repetition of the foregoing and complete investigation of the subject by Pasteur in 1862. The object of this paper is to make these last experiments more widely known; unfortunately they must be stripped of detail, and thereby robbed of much of their strength of argument. Few persons are familiar with the mode of experimenting, the facts observed, and the remarkable chain of evidence afforded by these most carefully-executed, most complete, and therefore most trustworthy, experiments.

Pasteur's Microscopic Examination of the Solid Particles Diffused in the Atmosphere.

The question which Pasteur first set himself to answer was, Is it possible to gain an approximate idea of the relation a volume of ordinary air bears to the number of germs that the air may contain? Let us see what means were taken to determine the number and the nature of floating particles diffused in the air.

By means of a water aspirator air was drawn from a quiet street, and also from the gardens of the Ecole Normale, in Paris, at some distance from the ground, through a tube containining a plug not of cotton-wool, as in the experiments of Schrœder, but of soluble pyroxyline, such as is used for making collodion. The amount of air aspirated in a given time was accurately measured, and after a sufficient interval the soluble cotton plug was removed and treated with its solvents, alcohol and ether. After allowing the dust to subside in a tube, the collodion was syphoned off, and more alcohol and ether added to effect the perfect removal of the collodion. The completely-washed dust was placed on a microscope slip and examined in a drop of water. By ordinary methods the action of different reagents, such as iodine water, potash, sulphuric acid, and colouring matters on the particles was tried. This process disclosed the fact that there is in ordinary air a variable number of corpuscles, ranging in size from extreme minuteness up to the diameters of 0·01 m.m. to 0·015 m.m.; some translucent particles of a regular shape so closely resemble the spores of the most common fungi that the most able microscopist could see no difference in them. The corpuscles were evidently organised, resembling completely the germs of the lowest organisms, and so diverse in size and structure as to belong without doubt to very various species. The soluble cotton used was previously tested and found to contain no residue insoluble in alcohol and ether beyond a fibre or two. By exposing a plug of pyroxyline for twenty-four hours to a current of air passing at the rate of a litre the minute after a succession of fine spring days, it was found that many myriads of organised corpuscles were collected.

Experiments with Heated Air.

Although it appears there are in air organised corpuscles in great numbers which are indistinguishable from the germs of the lowest organisms, is it really a fact that amongst these there are particles capable of germination? This interesting question was answered in a conclusive manner. Firstly, the facts announced by Schwann were firmly established, although they had previously been attacked by Mantegazza, Joly and Musset, and Pouchet. The solution, sealed up in flasks, was one extremely liable to change; its composition was—

Water	100 parts.
Sugar	10 ,,
Albumenoid and mineral matters from yeast	0·2 to 0·7 parts.

Boiled for two or three minutes, and then placed in contact with air previously heated to redness, not a single doubtful result was obtained, although repeated at least fifty times; not a single trace of any organised production was seen even after eighteen months, keeping at a temperature of 25° to 30° C.; while, if the liquid be left to ordinary air for a day or two, it never fails to become filled with bacteria or vibriones, or covered with mould. The experiment of Schwann applied to this sugar solution is, therefore, of irreproachable exactitude. Schwann, however, did not always succeed so well as he wished, and the experience of Mantegazza and Pouchet was at variance with his general conclusions; even Pasteur himself in some experiments failed to preserve his liquids. These are the particular instances:—Five flasks of 250 c.c. capacity, containing 80 c.c. of the sugar solution, were boiled, and during ebullition sealed up. The points were broken under mercury, and pure gases in all cases but one let into the flasks. Organisms were found in every case after four days. In all these experiments, as in those likewise of Schwann, which were contrary to the result of his first experiment with extract of meat, it was the mercury that introduced the germs. In making such experiments with a mercury trough, preservation of the liquid will not always succeed, even if it succeeds sometimes. If the sugar solution be replaced by milk and treated by either of the methods above described, the milk putrefies. These results, so different and contradictory, find a natural explanation further on, but so far they are facts of a troublesome nature.

Germination of the Dust which exists suspended in the Air, in Liquids suitable to the Development of the Lowest Organisms.

The facts ascertained so far are:—

1. That there exist suspended in the air organised corpuscles exactly like the germs of the lowest organisms.

2. That sugar solutions with the liquor from beer yeast, a fluid extremely alterable in ordinary air, remains unchanged and limpid, without even giving rise to infusoria or fungi, when left in contact with air previously heated.

The question now arises, how is it possible to sow an albuminous sugar solution with germs collected by means of pyroxyline in the manner already described?

Taking a flask containing such a sugar solution kept at 25° to 30° C. for one or two months unchanged, in contact with previously heated air, the sealed-up end is connected by means of a caoutchouc tube with one part of a T tube, while another is in connection with an air-pump, and a third with a platinum tube heated to redness. Between the T tube, however, and the flask is a wide tube containing a very narrow one within it, holding a plug of gun-cotton, through which a large volume of air has been passed. The tap in connection with the heated platinum tube was closed, and the one in connection with the air-pump opened; after exhausting air was admitted through the red-hot platinum, the tap was closed, and the air again pumped out, fresh air being admitted through the heated tube; this was repeated three or four times. The stop-cocks were then closed, and the sealed beak of the flask was broken within the india-rubber connection; the plug of gun-cotton was shaken into the liquid, after which the flask was sealed up again. All experiments so performed resulted in the liquid, after three or four days' exposure to a temperature of 25° to 30° C., decomposing, and being found to contain bacteria, vibriones, and fungi, just exactly like those in flasks exposed to ordinary air. There was no difference in the length of time requisite for the change, the forms of life occurring, or the nature of the change resulting in flasks so treated, and those with the same liquid exposed to common atmospheric air. These experiments can scarcely be surpassed for beauty in their arrangement, or for the importance and clearness of the evidence they afford. Yet thinking that it might be objected that the gun-cotton had given rise to the changes produced, Pasteur made use of plugs of asbestos, and found a like result; but when the plugs of asbestos were heated red-hot previous to being put into the flasks, the liquids remained unchanged in every case, and so constantly and with such perfect exactitude after an immense number of trials did the results remain the same, that the experimenter himself was astonished.

Extension of previous results to other very alterable Liquids—Urine, Milk, and Albuminous Sugar Solution mixed with Carbonate of Lime.

The facility with which urine exposed to the air becomes altered, and the change which takes place is well known. It becomes turbid and alkaline, sometimes filled with bacteria, or covered with patches of mucor or *Penicillium glaucum*. Often there is formed, when the temperature is not higher than 15° C., a pellicle consisting of a remarkable mucor closely resembling *torula*, but which is believed by Pasteur to be a different species. It consists of transparent cells, often without a nucleus, and considerably smaller than the cells of beer-yeast. There is also present in urine, when alkaline from the carbonate of ammonia resulting from the changed urea, a peculiar fungus in necklace-like groups, and this organism Pasteur is fully persuaded is the cause of urea being converted into carbonate of ammonia. An interesting observation was made with regard to the turbidity of liquids, which generally is the first sign of alteration; this is caused not merely by the presence of minute organisms, such as bacteria, but by their movements in the liquid; for when they are dead they settle to the bottom of the vessel, and the liquid becomes clear again. Many flasks of urine were treated in the manner already described —that is to say, they were boiled, and heated air was admitted to them. After preservation for months at 25° to 30° C. without change, plugs of asbestos through which air had been drawn were introduced; and then in cases where the liquid was alkaline, strings of this peculiar fungus were found invariably, and crystals of ammonio-magnesian phosphate were deposited. It was observed that *Bacterium termo* appears in a liquid before any other organism. This infusorium is so small that it would be impossible to distinguish its germ; but even if the appearance of its germ were known it would be still less possible to recognise it among the various particles of organised dust collected from suspension in the atmosphere.

In experimenting with milk boiled in flasks and exposed to heated air, it was found that generally in from eight to ten days, but in one case after so long a time as a month, the milk was found to be curdled. Microscopic examination showed that the whey was filled with vibriones, often of the species *Vibrio lineola*, and bacteria. The air of the flasks showed that the oxygen was replaced by carbonic acid; yet swarms of these vibriones were living in an atmosphere without oxygen. The most important observation which leads to an explanation of the extraordinary behaviour of milk in these experiments, is the fact that no mucor, torula, or penicillium—nothing but bacteria or vibriones—were found in the liquid. The obvious conclusion is, that these organisms or their germs are not destroyed by a temperature of 100° C. when the heated liquid which serves to develop them enjoys certain properties. To test this supposition, the milk was boiled under pressure, so that the temperature was raised during ebullition to 110° C., and then heated air was admitted, of course at the usual atmospheric pressure; flasks treated in this way were kept an indefinite period without the production of any life whatever. The milk preserved its flavour, its odour, and all its properties. Sometimes a slight oxidation of fatty matter took place, as could only be expected in such a considerable body of air; this was proved by an analysis of the air. In such cases the milk had a slightly suety taste. But what condition prevents the development of vibriones in sugar solutions and urine when heated to 100° C.? It is the fact that they contain a trace of acid. Milk is an alkaline liquid. If a liquid of the following composition:—

Sugar	10 grms.
Yeast water	100 c.c.
Carbonate of lime . . .	1 grm.

be boiled in flasks at 100° C., filled with heated air and sealed up and left to itself at 25° to 30°, in from two to four days it becomes turbid from vibriones, which have a very lively motion. It was found that a species of mucor after a time covered the surface of the liquid. It seems, therefore, that under these particular conditions, that the germs of this cryptogam had resisted the temperature of boiling water. An important confirmation of these experiments regarding the failure of a temperature of 100° C. to destroy certain germs here follows. Milk which had been preserved some months had a plug of asbestos presumably containing germs introduced into it by the manner already described; it was sealed up, and the flask was then plunged into boiling water; in eight days bacteria and vibriones were found in swarms. It was further discovered that 108° was too low a temperature to effect the preservation of these liquids.

It cannot be too forcibly impressed on the reader by what means and with what success Pasteur demonstrated the fact of myriads of organisms occupying comparatively small volumes of air. This is a point to which his detractors have willingly made themselves blind; they tell us the organisms are few in number without any experimental proof; while, on the other hand, Dr. Angus Smith and Mr. Dancer estimated that there were 37½ millions of organisms, many of which were recognisable, in 2500 litres of Manchester air.*

Another Method for showing that all the Organisms produced by previously heated Infusions have for their origin the particles which exist suspended in ordinary Atmospheric Air.

Says Pasteur, "I believe it to be rigorously established in the preceding chapters that all the organised productions of infusions previously heated, have no other origin than the solid particles which are always carried in the air and left deposited constantly upon everything. Could there still remain the least doubt of this in the mind of the reader, it will be dissipated by the experiments I will now describe."

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* Air and Rain, p. 505.

The experiments consisted in placing in glass flasks the following liquids, all of which are very changeable in contact with ordinary air, yeast liquor, sugar solution and yeast liquor, urine, beet-root juice, and infusion of pears; the flasks were then drawn out so as to have a long neck with many bends in all directions. The liquid is boiled for some minutes, while the steam escapes plentifully from the open neck; the flasks are then left to themselves without being sealed, and, strange to say, though the air enters, the liquid may be preserved for an indefinite period—an interesting fact for those who are accustomed to make experiments of such a delicate nature as this subject requires. There is no fear of transporting these flasks from place to place, or submitting them to the varying temperature of the seasons; the liquids show not the slightest alteration in taste or smell; they are truly specimens of Appert's food-preserving process. In some cases there was a direct oxidation of the matter, a purely chemical process. But it has already been shown how this action of oxygen was *always limited when organised productions were developed in liquids.* The explanation of these new facts is, that the air on first entering comes in contact with water vapour at the temperature of 100° C., and is so rendered harmless; what follows enters but slowly, and leaves its germs or particles of active matter in the moist curvatures of the tube-neck. After remaining many months in a warm place, the necks of the flasks are cracked off by a file-mark without other disturbance, and in twenty-four hours to thirty-six or forty-eight, fungi and infusoria make their appearance in the usual manner.

The same experiments can be made with milk, but then the milk must be boiled under pressure; milk has been kept for months in these open flasks without change at a temperature of 25° to 30° C. The production of organisms can always be started in these flasks by briskly shaking the liquid or by sealing during ebullition, and after cooling allowing the air to enter suddenly by breaking the point of the tube.

Many such flasks, exhibited at the Academy of Sciences, were preserved with their contents unchanged for eighteen months, although extremely prone to decomposition.

"The great interest of this method is, that it unquestionably proves that the origin of life in infusions which have been boiled is solely due to solid particles suspended in the air. Neither a gas, divers fluids, electricity, magnetism, ozone, things known or hidden causes, there is absolutely nothing in ordinary atmospheric air which, failing these solid particles, can be the cause of the putrefaction or fermentation of the liquids which we have studied." It has so far been definitely proved by Pasteur, and stated in the following manner:—

"1st. That there are constantly, in ordinary air, organised particles which cannot be distinguished from the true germs of the organisms found in infusions.

"2nd. When these particles and the amorphous *débris* associated with them are sown in liquids, which have been previously boiled and which remained unchanged in air previously heated, there appear in these liquids exactly the same forms of life as arise in them when they are exposed to the open air."

"Such being the case, could a partisan of spontaneous generation wish to uphold his principles even in the face of this double proposition? He might, but then his argument would necessarily be of the following kind, of which I leave the reader to judge for himself. There are in the air, he might say, solid particles, such as carbonate of lime, silica, soot, fibres of linen, wool, and cotton, starch granules . . . and besides these organised corpuscles having a perfect resemblance to the spores of the Mucedineæ or the germs of Infusoria. I prefer to attribute the origin of Mucedineæ and Infusoria to the first amorphous substances rather than to the second."

This has actually been asserted. Could there be more eccentric reasoning? Reasoning it is not. That question is beyond the pale of argument, to which common sense dictates the answer.

It is not exactly true that the smallest quantity of ordinary Air gives rise in an Infusion to the Organisms peculiar to this Infusion. Experiments on the Air of various Localities. Inconvenience of employing Mercury in Experiments relative to Spontaneous Generation.

If the smallest quantity of air in contact with an infusion gives rise to organisms, and these organisms are not of spontaneous origin, then it follows that in the minute portion of air there must exist a multitude of the germs of very different organisms; in such numbers, too, that, as Pouchet says, the air would be so loaded with organic matter as to form a thick fog. Strong as this reasoning is, it would be still stronger if it were shown that different forms of life are derived from different germs : this may be so, but it has not been proved.

Experimental proof of this statement, the error in which lies in gross exaggeration, was made by sealing up during ebullition flasks of 250 c.c. capacity containing about 80 c.c. of various liquids. On breaking the points of these flasks in certain noted places, the air entered with a rush into the empty space, carrying the germs along with it; after re-sealing, the flasks were placed in a warm situation and any change noted. In some cases the decomposition followed, and the production of the usual forms of life ; in other cases the flask remained as if they had been filled with heated air, quite unchanged. In two experiments made in the open air after a slight shower in the month of June, both resulted in the production of organisms; in four others, after a heavy rain in the same place, two of the flasks had their contents remain unchanged for at least thirteen months afterwards. These experiments were made, it is easily seen, in an agitated air, but Pasteur carried his labours into the cellars of the Paris Observatory, where the air is quite still except when agitated by the movements of the experimenter, and in that region below the surface of the earth where the temperature is unaffected by the changes of the seasons. It is to be expected that air, in which there is so little to cause its disturbance, would have deposited on the ground the germs which at one time floated in it. A greater proportion of flasks therefore, if opened and re-sealed in such an atmosphere, should have their contents preserved. Out of ten experiments made under such conditions with yeast water, in only one was any living thing found ; while eleven experiments made in the court-yard of the observatory at a distance of 50 centimetres from the ground, and at the same time, rendered in every case the usual forms of life ; a modification of these trials was made by letting air into flasks of liquid at various mountain heights. Eighty-three flasks, prepared in the manner already mentioned, were experimented on : twenty of these were filled up with air at the foot of the heights which form the first plateau of the Jura ; twenty others on one of the peaks of the Jura, 850 metres above the sea-level ; and the remaining twenty were carried to Montanvert, near the Mer de Glace, at an elevation of 2000 metres. The result was, that of the twenty opened on the lowest level, eight contained organisms ; of the twenty on the Jura, five only contained any; and lastly, of the twenty filled at Montanvert, while a strong wind blew from the deepest gorges of the Glacier de Bois, one only was altered. The method of opening the flasks was to hold them above the head, with the point turned from the wind, and by a pair of iron forceps, which had just been heated in a spirit-lamp flame, the point was broken. The drawn-out point had been previously scratched with a file and heated; otherwise particles of dust adhering to the glass would have been carried into the liquid by the in-rush of air.

A remarkable and interesting fact connected with these experiments was, that on one occasion Pasteur opened his flasks, and, on account of not being able to see the flame of his lamp against the brilliancy of the snow, it was impossible to re-seal them; the flasks were necessarily carried back to the little inn at Montanvert to be closed up. Everyone of these flasks contained organisms after keeping for a short time. On the glacier then, there are no germs in the atmosphere, but at the neighbouring inn the air warms with life, and life from all parts of the world, brought by the travellers. On opening the flasks they were held above the head, so as to prevent the possibility of germs attached to the person being deposited in them.

Explanation of the Cause of Failure of the Experiments in which Mercury is used.

Flasks containing liquids which had been kept for a great length of time were connected with an air-pump and a red-hot platinum tube: after repeated exhaustion and re-filling with heated air, the communication was made between the flask and the platinum tube, and a globule of mercury taken out of a mercury trough in the laboratory, which had previously been introduced into the connecting-tube of india-rubber, was made to roll into the flask; on re-sealing and keeping for a few days, fermentation ensued in every case, just as certainly as when the asbestos plugs and the adhering germs were sown in similar liquids. This case leaves no doubt regarding the cause of failure of experiments in which the liquid comes in contact with mercury by the flasks being broken under the surface of the quicksilver.

There are other facts which Pasteur established, of great interest and importance in connection with the nutrition of ferments, mucors and vibriones. Instead of experimenting on milk, urine, or solutions containing the liquor from yeast, he made use of such an infusion as the following; that is to say, a mixture of perfectly definite chemical substances:—

Pure water	100 parts
Sugar-candy	10 ,,
Tartrate of ammonia . .	0·2 to 0·5 part
Ashes of yeast	0·1 part

On impregnating such a liquid, when supplied with heated air, with germs collected from the atmosphere, bacteria, vibriones, and mucors, &c., were soon developed; the albumenoid and fatty matters, the essential oils, and pigments belonging to these organisms being derived from the elements of the ammonia salt, the phosphates, and the sugar. These complete organisms were built up out of the material afforded by such a mixture of simple substances, a fact which is quite contrary to Pouchet's declaration that ovules or germs were evolved from a sort of vitality remaining in lifeless, or, rather, dead, matter—that is to say, matter deprived of life.

A solution consisting of—

Pure water	100 parts
Sugar-candy	10 ,,
Tartrate of ammonia . .	0·2 to 0·5 part
Yeast ashes	0·1 part
Pure calcium carbonate .	3 to 5 parts

showed much the same phenomenon, in fact, differing only by a more marked tendency towards the changes called lactic, viscous, and butyric fermentations; and all ferments, whether vegetable or animal, characteristic of these changes were produced, simultaneously or successively.

Prof. Tyndall, in 1870, gave us a means of investigation, supplementary to the microscope, and of extreme delicacy. Aided by Prof. Huxley, he proved that particles in a liquid, quite invisible under an object-glass readily showing bodies $\frac{1}{100,000}$ of an inch in diameter were revealed with the greatest ease by means of a beam of light. If the air were pure, a beam of sunlight travelling a darkened room would be invisible except where it struck upon the wall. It is the scattering of the light by the floating dust which makes the track luminous, the larger and more numerous the particles the greater the luminosity. Hydrogen, coal-gas, air passed through cotton-wool, and the air of still places, were found to be free from floating matter. The writer, who has devoted much attention to this subject since 1865, made use of this discovery to aid him in a very careful repetition of some experiments published by Dr. Bastian in "Nature" of June

30th, 1870. The following few lines are a slight sketch of the results; for particulars the reader must be referred to the "Proceedings of the Royal Society" for 1872, p. 140.

Tubes cleansed with the greatest possible care, and afterwards heated to redness, were filled with solutions of the same composition as those which it was said by Bastian gave rise to organisms *in vacuo* after heating to so high a temperature as 150° C.; the water and liquids were tested according to Prof. Tyndall's method with a beam of light. After keeping for twelve months, during which time, on frequent examination with a ray of light, no change was seen to have taken place, drops of the liquids were allowed to run on to slips of glass placed in a bell-jar of hydrogen, such being a space shown to be free from floating matter. The microscope, with a higher power than that employed by Dr. Bastian, showed the solutions to be free from all organisms; nevertheless, portions let out into previously heated flasks, in a few days invariably became charged with living things. The original tubes, to which only pure air had been admitted, were kept weeks and weeks, and still no signs of life were visible in them; some of these tubes are in existence now, and still in the same condition. Here, then, were liquids, first, kept *in vacuo*, secondly, in pure air, thirdly, in ordinary air, and only under the last condition did they become filled with life, and that happened in every case. Without wishing to reflect on the work of anyone, it is simply stating a matter of fact to say, that results in favour of the theory of evolution *de novo* may be obtained most easily, and the more careless the experimenter the more successful would he be in that direction. We therefore see not only the extreme caution with which statements advancing heterogenesis should be received, but also the overbalancing weight of evidence contained in well-determined facts tending in an opposite direction.

VII. THE DOLMEN MOUNDS AND AMORPHOLITHIC MONUMENTS OF BRITTANY.

THE AMORPHOLITHIC LINES AND AVENUES.

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PART III.

MR. LUKIS deprecates the proneness of the native archæologists to dogmatise upon the intended uses and destination of these remains without a sufficient knowledge of their construction (from what we have already quoted, it will be seen that our theorists are not far behind the Breton *savants* in wild and ingenious supposition); and he partly agrees with Mr. Stuart, of Edinburgh, as to circles of stones not being temples, but sepulchral enclosures, but considers that as yet there is but insufficient evidence to show that the terminating circles of Menec and Kerlescant were used as burial-places, although Mr. Lukis himself found, in 1869, fragments of coarse clay vessels, flint scrapers, and chippings, within the area of the latter circle. Mr. Lukis comes to the following conclusion:—"*It is possible, therefore, that groups of pillars arranged in lines and circles, and associated together, may have served a purpose in some way connected with the funeral rites or solemnities that preceded interment.*" Since the above was written, Mr. Lukis has measured and planned a circle at Keswick; within this circle, and touching it, is an internal structure, which has every appearance of having served as a sepulchre; it may or may not be coeval with the circle, but Mr. Lukis's own impression is that it belongs to the original plan, and, if so, tends to confirm Mr. Stuart's view that these circles are sepulchral. It is a well-attested fact that many of the "Motes" and "Things" in Scotland were surrounded with circles of monoliths, sometimes termed "raises." That many of the circles and lines in Scotland are connected with sepulchral remains appears evident from Sir Henry Dryden's account of the following lines and circles—

"Lines, Battle Moss, Yarhouse.
Lines and cist, Garry Whin.
Lines, 'Many Stones,' Clyth.
Lines, Camster.
Circle ? Achanloch.
Circle, Guidebest, Latheronwheel.

"The groups of lines in France (of far larger stones and greater length than those in Caithness) have the largest stones, and widest intervals and the highest ground (the heads), to the W., or thereabouts, and the smallest stones, and narrowest intervals and lowest ground (the tails), to the E., or thereabouts. The Caithness groups differ entirely in principle. The one at Yarhouse loch runs N. and S., does not radiate, and is on nearly level ground; but the three others have the narrower intervals and higher ground to the N. (which end we may call the head), and radiate towards the S. and lower ground. The group at Battlemoss, near Yarhouse, is on ground falling slightly to N.W. It consists of eight lines placed N. and S. The width at the S. end is 44 ft. The lines are somewhat irregular, and appear to radiate slightly towards the N., but this is uncertain. One line extends 384 ft., and another one 170 ft., but the remaining six now only extend 133 ft. The ground is covered with peat and heather, and other stones may be hidden below the surface. There is no cairn or other grave now visible in proximity to the lines. The largest stones are about 2 ft. 6 in. high, 2 ft. 6 in. wide, and 1 ft. 3 in. thick.

"The group at Garrywhin consists of six lines. The whole width at the head (N.E. end) is 50 ft., and at the bottom 107 ft. The central lines bears N.N.E. or S.S.W. The length of this line is 200 ft. The fall is 20 ft. to the S.S.W. At the head is a cist of slabs, 3 ft. 6 in. by 2 ft. 6 in., and 2 ft. 4 in. deep, placed E. and W. As this grave is on the highest point of the knoll, and as the lines commence at it, it is fair to presume that they are connected. In the cist were found ashes, pieces of pottery, and flint chips, but no bones. As the cist is between the third and fourth lines, it is fair to presume that there never were more than six lines.

"The group called 'Many Stones' has the head on the top of a knoll from which the ground falls on all sides. The lines are on the S. slope, and are twenty-two in number. The width at the head or N. end is 118 ft., and at the bottom is 188 ft. The length in the centre is 145 ft., but there is no proof that this was the original length, and the presumption is the reverse. The average bearing is N. and S., and the fall 10 ft. 3 in. The largest stones now remaining are about 3 ft. high, 3 ft. wide, and 1 ft. 6 in. thick. There are numerous blocks of stone lying about the head, where, however, the rock is exposed, but the example of Garrywhin makes it probable that a cairn once existed on this knoll. There are no traces of any *sunk* grave, but the cairn may have contained a chamber above ground, like many in the vicinity.

"The group at Camster is on the moor, on ground falling slightly to the S.W. A considerable depth of peat overlies the rock here, and many stones are below the surface. There are now six lines ascertained. The length is 105 ft., width at the head, or N. end, 30 ft., and at the tail, or lower end, 53 ft. The average bearing is N. and S. The stones are smaller than at the last-mentioned group. There is no cairn or other grave apparent close to these lines, but in a direction due N., at 346 ft., is a cairn. No stones are now traceable between; but as there are gaps in the lines themselves, this blank interval may once have had lines on it to connect the cairn with the existing group. No habitation *now* exists near the spot, but there were many in this strath, which may account for destruction of stones in former times. A few hundred feet farther N. is the huge horned cairn described by Mr. Anderson, and at 436 ft. N.N.E. from the small cairn is the round chambered-cairn described in the same paper."

Mr. Barnwell writes as follows:—

"In North Wales is a remarkable example of a circle and avenue, unnoticed by Pennant and other writers. The description of it is given by Miss Davies, of Penmaen Dovey, the daughter and representative of one of the most accomplished scholars and judicious antiquaries of Wales. It is situated between two streams, called Cwym-y-Rhewi and Avon-y-Disgynfa, looking down from a considerable elevation on the Vale of Mochnant, and two miles above the well-known waterfall of Pistill-y-Rhaiadr. It consists of a large circle of isolated stones, of which thirteen were remaining when Miss Davies last saw it, and an avenue of two rows still retaining thirty-nine, and many portions of others that had been broken up. In the centre of the circle is a deep hollow, the site, no doubt, of the sepulchral chamber. The name Rhos-y-beddau, or *the graves on the moor*, has rescued the monument from being claimed by the Druids. The avenue appears to lead directly into the circle, the breadth of it corresponding to the space between the two stones of the circle where the circle and avenue meet, but it is probable that a stone or two is wanting at this part of the circle.

"In the northern part of Pembrokeshire is a single line of stones of great size, which Fenton does not mention, although he deliberately pulled to pieces a fine cromlech near it, and which seems to have been connected with this row of stones, for it was probably continued further northwards than it is at present. On referring to the Ordnance Map, a little to the right of the word 'Llanlawer,' will be seen the position of the line called in the map 'Parc-y-marw' (field of the dead); and a little further to the east, but slightly to the north, is marked down the cromlech destroyed by Fenton, and of which only some small fragments remain. The line of stones is parallel to the narrow road, and if continued would pass within a few paces of the ruined cromlech. Here, as at Rhos-y-beddau, the name points to the character of the monument; for experience has shown that local names of this kind in Wales, handed down from time immemorial, may be generally depended on. Local tradition, however, adds an account of a desperate battle fought on the spot, among the pillar-stones themselves, as if the possession of them were said to have been the sole object of the combatants. A lady, clad all in white, appears to those who are rash enough to walk that way by night; and so ancient is this tradition, which is still firmly believed, that a short distance before the stones commence, a foot-path, by long use now become public, turns across the fields to the left, making a *détour* of nearly a mile before it leads again into the road. During day-time the peasants do not think it necessary to take the roundabout course. The road itself is evidently one of great antiquity, and apparently led to the great work at Dinas. The height of the stones is not so striking, as their lower part is embedded in the tall bank of earth that does the duty of an ordinary edge; but some of them are full 16 feet long."

Mr. Lukis having shown conclusively that the lines of Carnac constitute not one monument, but three distinct groups, proceeds to compare them with Avebury. He remarks that now there is very little clue to its original plan, and that we are compelled to accept the inaccurate drawings of antiquaries of the seventeenth and early part of the eighteenth centuries. Whilst he confesses himself sceptical with regard to the ground-plan of Avebury as given by Stukeley, his doubt is strengthened by his intimate acquaintance with the Carnac and other groups of stone lines in Brittany. He prefers the more careful drawing in the plans of Aubray to the fanciful restoration of Stukeley, and gives as his opinion that the remains at Avebury were originally three distinct monuments; viz., one group of concentric circles, and short avenue, on Overton Hill; the second, of the larger circles and avenue of Avebury; whilst the third monument of like character, *i.e.*, composed of rows of stones associated with a circle, lay on the Beckhampton side. Mr. Lukis, however, feels that he has very little evidence in support of his views, with which, however, he will find many archæologists ready to agree. Beyond the fact that in both the Avebury and Carnac remains circles are associated with avenues, he finds the points of resemblance few and faint, and the points of dissimilarity numerous and strong: however, as one point of resemblance, he states that in Brittany the circular enclosure is invariably situated on an elevation, or on the summit of gently rising ground. In Wiltshire, one set of concentric circles is on Overton Hill, and the great circle of Avebury is also on a gentle elevation. Thus far, although the comparison of Avebury has not done much towards the elucidation of Carnac, yet the example of Carnac has taught us to look at Avebury in a new light.

Among the points of dissimilarity are the following, viz.:—At Carnac there are many—ten, eleven, and even thirteen rows of stones; at Avebury there were never more than two. With the Brittany circles there is no vallum or fosse, nor are there any concentric circles, all of which features appear to be characteristic of the Wiltshire remains.

Sir Gardner Wilkinson describes the stone lines of Dartmoor as leading up to concentric circles with cromlechs or kists, and as therefore being in some way connected with sepulchral and religious rites. Again, Mr. Spence Bate, in his supplementary report on the prehistoric remains of Dartmoor, mentions an extensive avenue in the neighbourhood of Corydon Ball, consisting of seven or eight rows, extending at least a hundred yards, with suggestive traces of what may have formed portions of a circle at the eastern extremity. A huge cairn, with a portion of a kist, are also mentioned near the same locality. It would be interesting to compare the seven or eight rows of stones at Corydon Ball with those described in this paper as to their parallelism or convergence, &c.

There are systems of avenues of stones with circles in various other parts of the world—in Lombardy, Africa, India, &c. We may quote the elaborately ornamented megalithic avenues leading to the tombs of the emperors of China as modern developments of the primæval structures. Thus we read that the great tomb (the Ling or resting-place of Yung-Lo, of the Ming dynasty), thirty miles from Pekin, consists of an enormous mound or earth-barrow, covered with trees. Its height is not mentioned, but it is evidently considerable, from the fact that the circular wall which surrounds it is a mile in circumference. In the centre of the mound is a stone chamber containing the sarcophagus in which is the corpse. This chamber or vault is approached by an arched tunnel, the entrance to which is bricked up. This entrance is approached by a paved causeway, passing through numerous arches, gateways, courts, and halls of sacrifice, and through a long *avenue* of colossal marble figures, sixteen pairs of wolves, kelins, horses, camels, elephants, and twelve pairs of warriors, priests, and civil officers. Whether this avenue is orientated or not is not noticed, but an idea of the size of these colossal marble figures may be formed from the following:—"During the building of the late Emperor Heen-fung's tomb, a road one hundred miles long was made from the quarries of Fang-shan to the Tung-ling, and a block of marble fifteen feet long, twelve feet high, and twelve feet broad, weighing sixty tons, was seen by several of us then resident in Pekin, being dragged along this road on a strong truck or car drawn by six hundred mules and horses." . . . . "This block was to be cut into the figure of an elephant to be placed as one of the guardians of the tomb."—(W. Lockhart, Proc. R. G. S., 1866).

Similarly, near Nankin, there exist avenues of colossal stone figures, attributed to the same Ming dynasty, in connection with the tombs, but what these tombs consist of is not mentioned. More south, in Fokhien, and doubtless throughout southern China, are found the horse-shoe or omega-shaped tombs which in some cases are associated with analogous approaches. Although not covered by artificial tumuli, the sepulchral chambers are excavated in the side of the natural hills, whilst those belonging to high officials are approached through avenues of stone pillars and carved figures, animal and human, although on a much smaller scale than those of Pekin and Nankin. A sketch of a group of these tombs, said to be those of former governors of Canton, at the foot of the White Cloud mountains, is exhibited.

Now we may venture to assume that all cromlechs, dolmens, kists, and other sepulchral stone chambers of every description, were originally covered with tumuli. Some of the tumuli appear to have had their bases strengthened by revetments or boundary walls of large upright stones. In Great Britain and the Channel Islands we frequently find that the tumuli have disappeared, leaving the structures thoroughly denuded of the smaller stones, earth, or sand which originally covered them, whilst the large blocks forming the revetment remain, and have been generally termed "*peristaliths*." These features certainly are unusual in Brittany, where, however, there are some examples,—at Kerlescant, Plouneour, and elsewhere. Now lately the author ventured to suggest that the circles of stone in Brittany and elsewhere might be looked upon as the possible remains of colossal "*peristaliths*," the sole indications of gigantic tumuli which may formerly have filled their interior space, and which have now disappeared by atmospherical, aqueous, and human agencies during the lapse of centuries. Nor need we much wonder if no trace of the actual sepulchral chambers within be left, when we consider that the largest tumuli have generally been found to contain the most insignificant kists; besides, it is far from improbable that the builders of the huge mounds, such as those at Mont St. Michel, &c., in the immediate neighbourhood of the lines and circles, constructed their barrows from the material afforded by the *débris* of the more ancient tumuli within the circles.

Mr. Fergusson, in his recent work on "Rude Stone Monuments," gives John Stuart ("Sculptured Stones of Scotland") the credit of having first remarked—"Remove the cairn from New Grange and the pillars would form another Callernish;" but thirty-seven years ago Mr. Leshingham Smith * notices the ingenious suggestion of the Messrs. Anderson, viz., that "*the circles usually called Druidical temples are nothing more than cairns without the loose stones*."

Since, however, the above suggestion was offered by the present writer to the late Ethnological Society, he (the author) is altogether inclined to admit the conclusion to which Fergusson has arrived, viz., that the stone circles in Europe appear to have been *introduced in supercession* to the circular earthern mounds which surround the early tumuli of our downs. These earthern enclosures still continued to be used surrounding stone monuments of the latest ages, but, if Mr. Fergusson is not mistaken, also gave rise to the form itself. For instance, the circle at Stanton Moor—called the nine maidens—may be looked upon as a transitorial example.

The circular mound, which is thirty feet in diameter, enclosed a sepulchral tumulus, as was no doubt the case from time immemorial, but in this instance was further adorned and dignified by the circle of stones erected upon it. A century or so afterwards, when stone had become more recognised as a building material, the circular mound may have been disused, and then the stone circle would alone remain. Fergusson also figures a woodcut, taken from Haxthausen's work of the uncovered base of a kurgan or tumulus at Nikolajew, in the government of Cherson, which he suggests may give us a hint as to the genesis of circles. The tumulus was cleared away, and its base was found to be composed of three or four concentric circles of upright stones surrounding what appears to be a kist of five stones in the centre. Similar arrangements have been found in Algerian tumuli, and it looks as if the first kist of the sepulchral circle may have arisen from such an arrangement having become familiar before being covered over, just as Fergusson supposes the free-standing dolmen to have arisen from the uncovered cist having excited such admiration as to make its framers unwilling to hide it. In fact, just as the free-standing dolmen and cromlech may be looked upon as the skeletons of original chambered tumuli after the flesh of the sepulchral mound, which gave meaning to the structure, had disappeared, so we may look upon the circle as the representative of the revetting peristalith which formerly encircled the tumulus, but which tumulus was ultimately never filled in; and similarly, we shall not be far wrong in looking upon the avenues which lead to circles as a development of the funnel-shaped narrow entrances to these same chambered tumuli. That they were intended for permanence is evident, and the people who erected them must have had similar associations of ideas regarding life and death as had both the Egyptians and Buddhists; the former, according to Diodorus Siculus, called the dwellings of the living mere "*lodging-houses;*" their tombs, on the contrary, they looked forward to as their "*eternal homes*."

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* *Vide* "Excursions through the Highlands and Isles of Scotland in 1835 and 1836," by the Rev. C. Leshingham Smith, M.A., Christ's College, Cambridge.

Anyhow, whether there were actually tumuli or not within these circular enclosures, the sepulchral theory seems the most fitting conclusion to arrive at; and if this be so, then the avenues may be looked upon as approaches of a ceremonial character connected with funeral rites, not necessarily only those which *preceded* interment, but for *subsequent* visitations, as shown by the permanent construction of these monuments, which were evidently intended to last through future ages.

As to this day in China the clans and families annually revisit the tombs of their ancestors for the purpose of worship and sacrifice, repairing and cleaning the graves, and placing food for the dead, &c., so through the alignments of Brittany may have passed at stated periods of time to do honour to the resting-place of their forefathers, the descendants of those whose bones rested within the sepulchral circles.

That there is some connection, as regards the funeral rites practised from ancient times, by the most orthodox of the Chinese, with *oriculation* and *stone pillars* appears plain from the following, taken from "The Life and Teachings of Confucius," by J. Legge. "According to the statutes of Hea, the corpse was dressed and coffined at the top of the eastern steps, treating the dead as if he were still the host. Under the Yin the ceremony was performed between the *two pillars*, as if the dead were both host and guest. The rule of Chow is to perform it at the top of the *western* steps, treating the dead as if he were a guest."

That the custom of surrounding the sepulchres of mighty kings is of remote origin throughout the East is evident from what we know of the funeral ceremonies practised at the time of the invasion of Western Asia by the Scythians, 625 B.C. Thus the *Chakravartins*, a branch of the great Scythian race, or Sakas, were styled the Wheel-kings—in fact, *Kings of the Circle—i.e.*, monarchs who ruled all within the *chakra* of rocks supposed to surround the world. Hence, as the symbol of universal authority, the tombs of these kings, after their cremation and certain recognised ceremonies, were surrounded by a circular range of rocks or unhewn stones—in fact, *amorpholiths*, to signify that they were Lords of the Universe. So Sakya Buddha requested that he should be buried according to the rules of the Chakravartins, *i.e.*, that his remains—after undergoing certain prescribed ceremonies—should be burned, and his tomb erected in the method known among the Sakas or Sakyas, viz., by raising over his ashes a vast mound of earth, and surrounding it with the usual emblems of authority—the circle of amorpholiths. How fully this rule was attended to in the erection of topes or stûpas is too well known to need illustration. These topes or stûpas were at first only mounds of earth, included within a circular wooden rail or ring of steles, as we find in India and Ceylon. But when the munificence of Asoka was brought to bear on the subject, these old and barbarous mounds were destroyed, and topes faced with stone—in many instances magnificently wrought and ornamented—came into date. But in these the original idea was never lost sight of; they are all designed to indicate the authority of a universal monarch—

not a monarch only of the world, but according to the expanded creed of Buddhism at the time of Asoka, lord of the "*three worlds*," also :—(1). The world of men, signified by the square plinth on which the dagoba rests surrounded by the circular rail. (2). The world of Dêvas, signified by the dome or vault of heaven; and (3). The world of space, signified by the kchêtra that rises from the Tce, ending in the symbol of the boundless empyrean—the three-forked flame or trisul. (Catena of Buddhist Scriptures, by S. Beal).

In Ceylon, the bell-shaped reli-shrines or Dagobas are surrounded with concentric circles of monoliths of various numbers.

Thus, at Thuparamya (250 B.C.), there are three concentric circles; and at another, on the hill of Mehentele, the concentric rows of granite pillars rise to half the height of the central mound. At Sandei and Amravati also we find the well known circles; at the former in the shape of stone imitation of wooden railings, and at the latter in two concentric circles of upright stones (193 feet outer diameter) carved with minuteness.

In India stone worship is very prevalent, and, in consequence, the custom of erecting amorpholiths is not yet extinct. In every part of Southern India four or five stones may often be seen in the ryots fields, placed in a row and daubed with red paint, which they consider as guardians of the field, and call the five Pandus. Col. Forbes Leslie supposes that this red paint is intended to represent blood. The god of each Khond village is represented by three stones. Col. Leslie gives the drawing of a group of sacred stones near Delgaum, in the Dekkan: the three largest stood in front of the centre of two straight lines, each of which consisted of thirteen stones. These lines were close together, and the edges of the stones were placed as near to each other as it was possible to do with slabs which, although selected, had never been artificially shaped. The stone in the centre of each line was nearly as high as the highest of the three that stood in front, but the others gradually decreased in size from the centre, until those at the ends were less than a foot above the ground into which they were all secured. Three stones, not fixed, were placed in front of the centres of the group. All the stones had been selected of an angular shape, with somewhat of an obelisk form in general appearance. The central group and double lines faced nearly east, and on that side were whitewashed: on the white, near, although not reaching quite to, the apex of each stone, was a large spot of red paint, two-thirds of which from the centre were blackened over. Dr. Hooker, too, remarks that among the Khasias "funeral ceremonies are the only ones of any importance, and they are often conducted with barbaric pomp and expense; and rude stones of gigantic proportions are erected as monuments, singly or in rows, or supporting one another like those of Stonehenge, which they rival in proportions." Major Godwin Austen describes some trilithons of the Khasias of immense size. The great stone of one of these monuments weighed 23 tons 18 cwt., and another is described as measuring 30 ft. by 13 ft. and 1 ft. 4 in. in thickness, and supported on massive monoliths. Mr. W. F. Holland also describes circles of massive stones as existing in the Peninsula of Sinai.

Mr. Fergusson has well shown how in India the tumulus has developed into the tope, and the tope into the temple. It is almost to be wondered at that he did not notice the extraordinary analogy between the groups, rows, and avenues of unhewn stones, and those thousand-pillared *chaöries* and *choultries* of the Southern Hindu temple-builders whose most important application is their use as *nuptial halls*, in which the annual mysteries sacred to the union of the male and female divinities are celebrated. Their other uses are, according to Fergusson, in his "Handbook of Architecture," most various—serving as porches to temples, as halls of ceremony, cloisters—where the dancing girls dance and sing—or as swinging-porches for the gods, who appear to have been pleased with such innocent amusement. At Tinnevelly, for instance, the great pillared hall has 100 columns in its length by ten in width, so that it would have 1000 pillars, were not twenty-four omitted to make way for a small temple.

At Chillumbrum, the hall is twenty-four pillars wide by forty-one in length, which, adding the sixteen of the porch, would make up the number; but some are omitted in the centre, to make space for ceremonies, so that the actual number is only 930.

At Seringham the hall is of about the same extent, and several other temples have halls, the number of whose pillars varies from 600 to 1000. In most instances no two pillars are exactly alike.

The temple of Tiruvalur measures externally 945 ft. by 701 ft. In the outer court, and towards the principal entrance, is the great *choultry*, intended apparently to have had 1000 columns, being sixteen pillars wide by forty-three in depth, *one half, however, of them support no roof*, so that the structure, according to Fergusson, is "*probably*" (or, as he says in another place, *evidently*) unfinished. If this great temple is really finished, as does not seem improbable, we have here some hundreds of carved pillars forming an approach of several avenues, which have been erected within recent times for a specific object.

Fergusson, it must be borne it mind, gives no reason for supposing that this edifice is unfinished, or that the architects ever contemplated putting a roof on these columns, and it is certainly well worth noticing, and enquiry should be made upon this subject.

The whole number of columns standing is 688; they are all equally spaced, except that there is a broad aisle down the centre, and a narrower transverse avenue in the direction of the entrance. Fergusson gives a plate (No. 65), taken from Ram-Raz's "Hindu Architecture," which shows the forest of pillars supporting no roof. Fergusson likens the great choultry to the Stoa Basilica of Herod's restored Temple.

Another analogy in the great development of stone avenues may be noticed in the avenues of sphinxes extending for miles on the banks of the Nile, connecting the Hypostyle Halls, Pylons, &c., of the palace-temples of great Thebes.

Fergusson's conclusion as to the age and destination of the Carnac stone rows may be summed up briefly as follows:—

(1.) That it is most improbable that a temple should extend over six or seven miles of country; in fact, he hardly knows any proposition that appears to him so manifestly absurd as that these stone-rows were temples, and he feels sure that no one who thinks twice of the matter will venture again to affirm it.

(2.) It seems equally clear that they were not erected for any civic or civil purpose. No meetings could be held, and no administrative functions could be carried on in or around them.

(3.) They are not sepulchral, in the ordinary sense of the term, as nowhere were men buried in rows like this, extending over miles of heath and barren country; moreover, the French *savants* have dug repeatedly, and found no trace of burial. "It no doubt is true that the long barrow at Kerlescant, the dolmen at Kermario, and the enclosure at Menec, may have been, *indeed, most probably were, burying-places*, but they can no more be considered the monument than the drums and fifes can be considered the regiment. They are only adjuncts; the great rows must be considered as essentially the monuments." (Why so?)

outlines and relations of the two branches of scientific investigation—*vulcanology* and *seismology*—by which its true nature and part in the cosmos are chiefly to be ascertained." He remarks, by way of defining his subject, that "Vulcanicity properly comprehends all that we see or know of actions taking place upon and modifying the surface of our globe, which are referable not to forces of origin above the surface, and acting superficially, but to causes that have been or are in operation beneath it. It embraces all that Humboldt has somewhat vaguely called "the reactions of the interior of a planet upon its exterior." He indicates the relation between astronomy and physical geology, which overlap each other, through vulcanicity. He then sketches the history and progress of knowledge in the chief domains of vulcanicity. In discussing the more recent contributions to the science, commencing with his early paper "On the Dynamics of Earthquakes," which appeared early in 1846, he takes occasion to point out that Mr. Hopkins, of Cambridge, in his Report "On the Geological Theories of Elevation and Earthquakes," read before the British Association in June, 1847, did him some injustice. He remarks, that if his paper be compared with Mr. Hopkins's Report, it will be found that as respects the earthquake part, the latter work parades in a mathematical dress some portion of the general theory of earthquake movements, previously published by Mr. Mallet. "This," he proceeds, "is but too mystifyingly suggestive of the 'Pereant qui mea ante mihi dixerunt'" (a somewhat novel rendering of the hackneyed quotation, by the way). We dwell on this point, because in Prof. Phillips's "Vesuvius," the injustice (unintentionally, of course) is continued, and the theory of earthquakes is too important a contribution to science to be handed over to one who certainly was not its author. The definition of an earthquake in Mr. Mallet's paper of 1846 sufficiently indicates the main teaching of his theory; an earthquake he there says, is "The transit of a wave or waves of elastic compression in any direction, from vertically upwards or horizontally, in any azimuth, through the crust and surface of the earth, from any centre of impulse or from more than one, and which may be attended with sound and tidal waves dependent upon the impulse and upon circumstances of position as to sea and land." The whole paper should, however, be carefully studied by those who wish to form a just opinion of the position in which Mr. Mallet stands with respect to the view of earthquakes, soon to become the established theory on the subject.

From the date of the publication of that paper until that of the paper recently contributed by him to the Royal Society, Mr. Mallet has continued his researches, experimental and mathematical, and the views to which he has been led may be regarded as affording, in the main, the most complete and satisfactory account of the phenomena of earthquakes and volcanoes yet extant. Passing over those portions of his views which relate to the period when our globe first liquefied from the nebulous condition, and to the earliest stages of cooling by radiation, when the crust was extremely thin, as also his account of the deformation of the spheroid as one of the first effects of its contraction, we find that he has endeavoured to show that the rate of contraction of the crust while very thin exceeded that of the large fluid nucleus supporting it, and so gave rise to *tangential tensions* in the crust, fracturing it into segments; but next, "that as the crust thickened, these *tensions* were gradually converted into *tangential pressures*, the contraction of the nucleus now beginning to exceed (for equal losses of heat) that of the crust through which it cooled. At this stage these tangential pressures gave rise to the chief elevations of mountain chains,—not by liquid matter by any process being injected from beneath vertically, but by such pressures mutually reacting along certain lines, being resolved into the vertical, and forcing upwards more or less of the crust itself. The great outlines of the mountain ranges and the greater elevation of the land were designated and formed during the long periods that elapsed in which the continually increasing thickness of the crust remained such that it was still, as a whole, flexible enough, or opposed sufficiently little resistance to crushing to admit of the uprise of mountain chains by resolved tangential pressures." "As our earth is still a cooling body, and the crust, however, now thicker and more rigid, is still incapable of sustaining the tangential pressures to which it is now exposed, so it is by no means inferred that (relatively) slow and small movements of elevation and depression may not still and now be going on upon the earth's surface; in fact, all the phenomena of elevation and depression, rending, &c., which at a much remoter period acted upon a much grander and more effective scale." "But the thickness of the earth's crust, thus constantly added to, by accretion of solidifying matter from the still liquid or pasty nucleus, as the whole mass has cooled, has now assumed such a thickness as to be able to offer a too considerable resistance to the tangential pressures to admit of its giving way to any large extent by revolution upwards; yet the cooling of the whole mass is going on, and contraction though unequal, both of thick crust and of hotter nucleus beneath also, whether the latter be *now* liquid or not." "For equal decrements of heat, or by the cooling in equal times, the hotter nucleus contracts more than does its envelope of solid matter. The result is now, as at all periods since the signs changed of the tangential forces, thus brought into play, *i.e.*, since they became tangential pressures; that the nucleus tends to shrink away, as it were, from beneath the crust, and to leave the latter, unsupported or but partially supported, as a spheroidal dome above it." Mr. Mallet shows that, in this state of things, and under the actual conditions to which the crust of the earth is subjected, this crust must *crush*, "to follow down after the shrinking nucleus. . . . . It must crush unequally, both regarded superficially and as to depth; and the crushing will not be absolutely constant and uniform anywhere or at any time, or at any of those places of weakness to which it will be principally confined, but will be more or less irregular, quasi-periodic, or paroxysmal; as is, indeed, the way in which all known material substances (more or less rigid) give way to a slow but constantly increasing steady pressure."

Such is a brief sketch of the general views of Mallet; but for the details, and particularly for the estimates of the rate at which the vulcanic processes now in progress are taking place, and an account of the experiments conducted to obtain these estimates, the reader is referred to the present work.

It is hardly necessary to point out how much the interest of Palmieri's narrative is enhanced by its association in this treatise with Mr. Mallet's inquiries into the phenomena of the earth's crust. In fact, Mr. Mallet has specially tested his views by a study of the phenomena presented during the last two thousand years by Vesuvius, "the best known volcano in the world."

Nevertheless, it is to be noted that Palmieri's Memoir contains much which does not bear directly on Vulcanology. It will be none the less interesting on this score, however, to the general reader; and we recommend all those who are desirous to learn all the circumstances of a great and characteristic eruption of Vesuvius, to turn to the pages of this book. As Mr. Mallet well remarks, "a special narration such as this should not suffer in popular estimation by the fact that Prof. J. Phillips has so recently given to the world the best general account of Vesuvius in its historical and some of its scientific aspects which has yet appeared."

Papers relating to the Transit of Venus in 1874. Prepared under the Direction of the Commission Authorised by Congress. Published by Authority of the Hon. Secretary of the Navy. Part I. Washington.

THESE papers consist of a series of letters on the subject, the most important being those from Mr. Rutherford, and of an essay "On the Application of Photography to the Observation of the Transits of Venus," by Professor Newcomb. Mr. Rutherford describes his method for photographing the sun as a guide to the method of photographing the phenomena of the transit; and then says—"If the whole matter of ordering instruments for the photographing of the transit of Venus were in my control, with my present lights, I should have an achromatic objective of five inches aperture, and seventy inches focus, in a cell which would allow of the application, in front of it, of a lens of flint-glass of such curves as would shorten the focal distance (for photographing) to sixty inches. At the proper point, I would place between the two distances an enlarging-lens so constructed that the normal image of the sun in the principal focus (then about half an inch) would be enlarged to two inches at the distance of ten inches from the principal focus, viz., at 70 inches from the objective. The camera-box and tube should be one tube, and the focalising rack and screw should be located at the objective end of the tube, thus simplifying the whole arrangement and permitting the use of braces, from end to end, to prevent flexure; and on taking off the photographic corrector, and taking out the enlarging-lens, the instrument will be all ready for vision. On consideration, I do not think I would counsel a smaller telescope than the one I have named."

Professor Newcomb divides the proposed methods of observing the transits of Venus into two classes. The first consists in fixing the moment at which the planet is in contact with the limb of the sun; the second, in determining the relative position of the centre of the planet and the centre of the sun as often as possible during the transit. The first method, although only that has hitherto been thought practicable, Prof. Newcomb conceives liable to inaccuracies; and he proposes photography as the aid to the second method, in order to form an image of the sun with Venus on its disc, so that points on the plates corresponding to the centres of the discs can be fixed with precision, the linear distance between these points being determined by means of a micrometer, and the angle of position obtained from a reference line—this line bearing a relation to the circle of right ascension passing through the sun's centre. For the details of the process of photographing the transit, the corrections necessary in the glasses, we must refer the reader to the original papers.

There are some considerable difficulties connected with this method. The greatest difficulty would appear to be, that the required element appears only as a minute difference between two comparatively long arcs, too long to be measured by a micrometer. But Professor Winlock's apparatus would remove many of the disadvantages.

These papers contain so much important matter that we hope soon to see the second part.

Memoirs of the Geological Survey of England and Wales. Vol. iv.: *The Geology of the London Basin.* Part I.: *The Chalk and the Eocene Beds of the Southern and Western Tracts.* By WILLIAM WHITAKER, B.A. (Lond.). (Parts by H. W. BRISTOW, F.R.S., and T. Mc. K. HUGHES, M.A.) London: Longmans, Green, and Co.; and Stanford. 1872.

IT is obviously a matter of convenience to the public that the Maps of the Geological Survey, as they are issued sheet by sheet, should be accompanied by short explanatory memoirs. But, since it necessarily happens that the areas comprised within these sheets are bounded in an arbitrary fashion, it becomes in the highest degree desirable that—as the work of the Survey progresses, and districts with well-defined natural limits are worked out—the information scattered through the shorter memoirs on the separate sheets should from time to time be gathered together and expanded into special volumes, each devoted to a full description of the geology of some extensive tract of country, bounded by well-marked physical features. Such a volume is the admirable memoir by Mr. Whitaker on the Geology of the London Basin.

Much misconception prevails respecting the true nature of this so-called "Basin." Misled by the popular use of the term, and accustomed to the caricatured sections given in most geological works, one finds it difficult to realise the very gentle nature of the trough in which the metropolis is seated, and the true dip and relation of the beds within the London area. But on studying the sections issued by the Geological Survey, which are drawn on the same scale horizontally and vertically, it is immediately seen that the disturbances which have affected the strata in the south of England have been of the tamest possible kind—that there have been no vast foldings of the beds, no great elevation here or depression there—and that such high-sounding phrases as "the great arch of the Weald," or "the deep trough of the London Basin," are equally deceptive. "When compared with its horizontal extent," says Mr. Whitaker, "the vertical displacement in the latter area is indeed trifling."

The chalk is the lowest formation exposed within the London Basin, though well-sections have reached the Upper Greensand, the Gault, and certain lower beds—perhaps of Neocomian age, or even older. Above the chalk come the Lower Eocenes, comprising the Thanet beds, the Woolwich and Reading beds, the Oldhaven beds, and the London Clay. It may not be amiss to remark the name "Oldhaven beds" was proposed by Mr. Whitaker, in 1866, for some sands and pebble-beds equally distinct from the London clay above and from the Woolwich beds below. These beds are well exposed at Oldhaven Gap, on the Kentish coast, near the Reculvers. Passing from the Lower to the Middle Eocenes, we find in the London Basin the Lower, Middle, and Upper Bagshot series; but the beds above these are not represented in the London area, and to study the Eocenes it is necessary to cross to the Hampshire basin. As to the various superficial deposits, they are well enough exposed, it is true, within the London basin, but it formed no part of Mr. Whitaker's plan to describe them, as it is proposed that they shall form the subject of the second part of this volume.

In the systematic preparation of this memoir, Mr. Whitaker's course has been to notice the several geological formations in ascending order—first describing their general nature, and then detailing their range, their lithological characters, and the sections in which they are exposed.

Having thus described the nature and range of the various formations, the author devotes one chapter to the disturbances which the beds have suffered, and another to the physical features which they present. In discussing the causes which have given to the country its present contours, Mr. Whitaker clearly shows that the surface has been sculptured into its present form of hill and scarp and dale by subaërial denudation, rather than by marine action—that, in fact, the varied features of the scenery are mainly, if not exclusively, due to meteoric agencies—to rain, rivers, frost, and the like—agencies which are ever silently at work under our eyes, and are fully competent to effect all that has been ascribed to their action, if only sufficient time be granted for the work.

The concluding chapter of the Memoir is devoted to Economic Geology—a subject which, in this area, does not admit of very extensive treatment. But perhaps the most valuable part of the work—as a work of reference—is to be found in its copious Appendices. One of these, on the Bibliography of the subject, shows in a remarkable manner Mr. Whitaker's extensive acquaintance with geological literature; whilst the second Appendix contains details of upwards of 500 well-sections and borings within the area under description. Finally, Mr. Etheridge and some other palæontologists contribute valuable lists of fossils from the beds of the London Basin.

Before closing the work we should remark that, though the great bulk of the text has been written by Mr. Whitaker, certain parts have been contributed by his colleagues—Mr. H. W. Bristow, F.R.S., Director of the Geological Survey of England and Wales; and Mr. T. McK. Hughes, M.A., the new Woodwardian Professor of Geology at Cambridge.

The publication of this elaborate volume leaves no longer any excuse for ignorance on the geology of the country around London. It must, however, be confessed that the physical features of the country within a moderate distance of the metropolis are not such as tend to foster geological tastes; and, in spite of the labours of Mr. Prestwich and of the Geological Survey, we fear that among the millions who dwell within the area of the London basin, there are comparatively few who know anything of the true nature of the ground beneath them. "Turpe est in patriâ vivere, et patriam non cognoscere."

The Theory of Strains in Girders and Similar Structures, &c. By Bindon B. Stoney, M.A. London: Longmans, Green, and Co. 1873.

The constantly growing demand for education, in every path in life, must soon have the effect of replacing those hard-headed, practical, self-taught, but untheoretical engineers who, it cannot be denied, have been the pioneers of the profession, and to whom credit is due for the construction of many important and magnificent works. But, as is stated by Mr. Stoney in his preface, "practice which was formerly excusable, or even worthy of the highest commendation, would, now that knowledge has increased, be properly described as culpable waste." At the present day, the engineer requires not only to be a practical man, but he should also be well acquainted with the physical laws by which his works are regulated, so that he may at once combine strength and refinement in his structural details.

Nothing can be more important, in connection with engineering structures, than a complete knowledge of the strength of the materials employed, and this again requires to be augmented with a full appreciation of the duty to be performed by each portion of such structures; in other words, of the strain or stress to which each such portion is subjected, and of its capabilities to resist it. The work now before us is a handbook to such knowledge, so far as iron structures are concerned, accompanied by observations on the application of theory to practice, and tables of the strength and other properties of materials, compiled from such authorities as Hodgkinson, Tredgold, Wertheim, Young, Fairbairn, Barlow, &c.

The principle of strains is based on the fact that on the application of force all bodies change either form or volume, or both together. For convenience sake such strains are divided under five heads, namely, tensile, compressive, transverse, shearing, and torsional strains, according as they are caused by tearing asunder, crushing, breaking across, cutting, and twisting asunder, respectively. As the strength of any material depends ultimately on its capability of sustaining strains, it is of essential importance to know the ultimate resistance to tension or compression which each material possesses, and thence deduce those strains which may be safely imposed in practice; and the object of the present work is to put before the student in this branch of science the results of the investigations, carefully worked out, by those who have given more particular attention to the subject. Besides the strains of tension and compression, the elongation and shortening of the material subject to strain claims attention, for experience has proved that the safe working strain of any material does not exceed its sensible limit of uniform elastic reaction, generally called the limit of elasticity. This limit may also be defined to be the greatest strain that does not produce an appreciable set.

The investigation of transverse strains may be reduced to the three following fundamental laws in mechanics, viz., the resolution of forces, the law of the lever, and the equality of moments, upon which are founded all the investigations given of the strength of materials when subject to transverse strain.

After an introductory chapter, our author enters upon a consideration of the circumstances of flanged girders with braced or thin continuous webs, when subjected to six different conditions of weight or strain. In this part of the work the formulæ investigated refer to transverse strains only, the horizontal strains in braced or thin continuous webs being so inconsiderable that they may be practically neglected. All girders have what is called a *neutral surface* and a *neutral axis*, the former being that surface along which the resultant of the horizontal components of all the diagonal forces equal cipher, and the latter the line of demarcation between the horizontal elastic forces of tension and compression exerted by the fibres in that particular section of the girder. The sum of the moments of the horizontal elastic forces in any transverse section round any point whatsoever is the *moment of resistance* of that particular section, or, as it is also called, the *moment of rupture*. The coefficient of rupture varies, of course, with different materials, and, in order to enable the formulæ given on the above subject to be the better applied, a table of coefficients is given.

As the result of several investigations, it is laid down as a rule that the strength of similar girders varies as the square of their lineal dimensions, but the weight of the girder itself varies as the cube of its lineal dimensions.

Space will not admit of our following Mr. Stoney's book in such detail as the interest of the subject would otherwise justify. The calculations given respecting one class of girders are continued to girders of various sections, to girders with parallel flanges, but having webs formed of various-shaped bracings, to girders with oblique or curved flanges, &c. The chapter on "Deflection of Girders" is an important one, showing, as it does, that girders of uniform section throughout are often defective from a scientific point of view. In all properly constructed girders each part is duly proportioned to the maximum strain which can pass through it, so that no material is wasted; and when this occurs in a girder with horizontal flanges and a uniformly distributed load, that is, the load which produces the maximum strain in the flanges, these latter will taper from the centre, where their section is greatest, towards the ends as the ordinates of a parabola.

Having considered and given rules for the quantity of materials, and the angle of economy for braced girders, there follow chapters on torsion, the crushing strength of materials, and rules for the strength of pillars, whether circular or braced. These, of course, have reference to the construction of piers and abutments, or other supports for girders, and are most important. The tensile strength of materials is very fully discussed, and is followed by chapters on *Shearing-Strains*, *Elasticity and Set*, and *Temperature*. Next follow chapters on the detail parts of girders and bridges, such as *flanges*, *web*, *cross-girders and platform*, *working load*, &c.; after which several pages are given to *estimation of girder work*, which forms a most fitting sequel to what has preceded. The book concludes with an appendix, in which many interesting and detailed particulars are given of the Boyne Lattice Bridge, on the Dublin and Belfast Junction Railway, which affords a practical illustration of the theories laid down in the main body of the book.

On the Cause, Date, and Duration of the Last Glacial Epoch of Geology, and the Probable Antiquity of Man. With an Investigation and Description of a New Movement of the Earth. By Lieut.-Col. DRAYSON, R.A., F.R.A.S., &c. London: Chapman and Hall. 1873.

IN spite of all that has been written—whether by geologist, astronomer, or physicist—in explanation of the different conditions of climate in past phases of the earth's history, the subject still remains so enshrouded in obscurity that light from any quarter should be gladly greeted. Perhaps the most remarkable—certainly the most interesting—of these climatic conditions is represented by that period which geologists recognise as the Glacial Epoch,—an epoch in which arctic conditions seem to have prevailed over the northern hemisphere down to at least the forty-fifth parallel of latitude. It is universally conceded that this episode in the history of our planet occurred in comparatively recent geological times, but we are as ignorant of its absolute date and period of duration as of the physical causes by which it was brought about. It is, however, to the solution of these problems that Col. Drayson addresses himself in the present work.

Rather than offer his own description of the Glacial Epoch, the author cites copiously from the writings of Ramsay, Lyell, Agassiz, Page, and other geologists. He then discusses and dismisses the several theories which have from time to time been advanced with the view of explaining the cause of these glacial conditions,—such as the passages of the earth, with the rest of the solar system, through zones of space of different temperatures; the assumed changes in the excentricity of the earth's orbit; differences in the distribution of the great masses of land and water; and alteration in the position of the earth's poles by shifting of the axis. It is strange that we fail to find here any reference to the writings of Mr. Croll, who has, of late years, so ably discussed some of these theories.

In introducing his own explanation, Col. Drayson begins by examining the three principal movements of the earth—its rotation on its axis, its revolution round the sun, and especially the slow movement of its axis round the pole of the ecliptic. It is almost universally laid down by astronomical authorities that the pole of the heavens moves in a circle round the pole of the ecliptic, as a centre, constantly maintaining an angular distance of 23° 28′ from that centre. The author seeks to refute this generally-accepted proposition, and endeavours to prove that the earth's axis describes a circle—not round the pole of the ecliptic as a centre, but round another centre 6° distant from the pole. As the full astronomical discussion of this movement is reserved for a forthcoming volume, we withhold criticism on this part of the work, and confine ourselves to the geological consequences which tend to flow from the author's data.

Assuming Col. Drayson's premisses, it follows that during one revolution of the pole of the heavens round the pole of the ecliptic, occupying about 31,840 years, there must be a variation of 12° in the obliquity of the ecliptic. This variation is sufficient to account for extraordinary changes of climate on the surface of the earth. It is calculated that at the date 13,702 B.C. the obliquity was at its maximum, namely, 35° 25′ 47″. At that time, therefore, the arctic circle would be brought down to this distance from the pole, and our own islands would consequently come within the frigid zone. But whilst our winters were thus characterised by arctic severity, the author argues that the summers must have been almost tropical. In winter, then, the country would be covered with a complete mantle of ice, and in summer this would be rapidly thawed, giving rise to heavy floods and vast numbers of icebergs.

We have seen that, according to our author, the Glacial Epoch was at its height in 13,700 B.C. He believes, however, that the occurrence of great alternations of temperature, producing marked effects on the climate, extended over a period of about 16,000 years—8000 before and 8000 after the maximum. The glacial period would, therefore, have begun in 21,700 B.C., and terminated in 5700 B.C.

Assuming the course of the pole to be uniform, there would be a recurrence of glacial periods every 31,000 years. Prof. Ramsay, from the study of certain beds of breccia, long ago insisted on the necessity of recognising earlier glacial periods; and the very phrasing of Col. Drayson's title, "The *Last* Glacial Epoch," shows that he, too, believes in previous periods of a like character. Few geologists will, however, agree with the author in his curious suggestion that these extreme climatic conditions may account for the alternation of different beds in our coal-measures, much less for the bands of flint in our chalk.

In closing Col. Drayson's work, the geological reader, though anxious to accept many of his conclusions, will feel that he must be guided mainly by the verdict of the astronomer. After twelve years of patient thought upon his favourite subject, the author ventures to maintain that certain time-honoured principles in Astronomy require correction. That he is thus bold enough to be original is no reason why his propositions should not be candidly discussed. Every new idea makes its way in the world with difficulty; and we hope that the mere novelty of the author's views, whether right or wrong, will not preclude him from a fair hearing by men of science. "The imputation of novelty," says Locke "is a terrible charge amongst those who judge of men's heads as they do of their perukes—by the fashion; and can allow none to be right but received doctrines."

The School Manual of Geology. By J. BEETE JUKES, M.A., F.R.S., &c. Second Edition, revised and enlarged. Edited by ALFRED J. JUKES-BROWNE, of St. John's Coll., Cambridge. Edinburgh: Adam and Charles Black. 1873.

WITH the exception of the classic writings of Sir Charles Lyell, there are perhaps no modern text-books better known to the student than the Manuals of the late Prof. Jukes. His were no mere compilations, as elementary treatises too often are, but were the work of a field geologist, whose heart was in his hammer. The success of Jukes's larger volume, the "Students' Manual," induced the author, about ten years ago, to write an introductory work, under the title of the "School Manual of Geology." Since the lamented death of Prof. Jukes new editions of both works have been called for; the editing of the larger Manual was entrusted to the author's colleague, Prof. Geikie; that of the smaller Manual to the author's nephew, Mr. A. Jukes-Browne.

Since the original appearance of the "School Manual" geology has made great advances. But whilst duty to the reader has compelled the editor to effect many alterations, he has wisely forborne, from respect to his uncle's memory, to unnecessarily interfere with the original plan of the work. Jukes's "School Manual" remains, then, what it has always been—one of our best text-books for the student of elementary geology.

Geological Stories; A Series of Autobiographies in Chronological Order. By J. E. TAYLOR, F.G.S., &c. London: Hardwicke. 1873.

JUDGING from the large number of "Play-Books of Science" constantly being issued, there must be a large section of the reading public desirous of acquiring a smattering of scientific knowledge without the labour of systematic study. To such readers Mr. Taylor's "Stories" will be peculiarly acceptable.

Written in a pleasing gossiping style, they lead the reader smoothly onwards, until he finds himself in possession of a great deal of geological information.

These stories have, for the most part, already appeared in "Science Gossip," a journal of which the author is editor; but they are now arranged in chronological order, so as to present a simple and picturesque view of the past history of our Earth. The autobiographies are told by pieces of granite, quartz, slate, limestone, sandstone, coal, rock-salt, jet, Purbeck marble, chalk, clay, lignite, the "Crags," a boulder, and a gravel-pit.

Whilst recommending these "Stories" to the class of readers for whom they were primarily intended, we cannot help remarking an unsatisfactory looseness of expression, common to most popular writings, but annoying to the scientific reader. For example, confining ourselves to the first chapter,—the story of a piece of granite,—we object to alumina, soda, potash, lime, and other oxides, being constantly called "elements;" nor are we pleased to hear the chemical constituents of mica and of felspar spoken of as "mixed" together in these minerals respectively. But the most curious statement in this chapter is that felspar may be detected in a mass of granite by being "so soft that you may scratch it with your finger-nail!" If this extraordinary assertion is made on the authority of personal examination, it is clear that either certain parts of the author's exo-skeleton had acquired an unwonted degree of induration, or the specimen under test was advancing to a state of kaolinisation. Mr. Taylor is evidently more at home when speaking of fossils than of minerals; and, as might be supposed, we find him at his best in the later chapters, from the "Story of the Crags" onwards.

Despite any little defects observable here and there, the work contains an attractive collection of stories well calculated to quicken a taste for geology in those who may be too careless about the grand Science of the Earth to apply themselves to the study of systematic treatises.

The Owen's College Junior Course of Practical Chemistry. By H. E. Roscoe, B.A., F.R.S., Professor of Chemistry in Owen's College, Manchester, and Francis Jones, Chemical Master in the Grammar School, Manchester. London: Macmillan and Co.

The number of elementary works on chemistry which have been lately issued from the English press proves that the importance of this science is at last beginning to receive something like due recognition. Amongst these treatises few are likely to prove more valuable than the one before us, which bears the impress of having been arranged by one who, like Prof. Roscoe, has learnt from prolonged experience what the student exactly needs. The synoptical tables for the detection and qualitative separation of the elementary bodies, when occurring in compounds or mixtures, are well arranged. The student who can give a correct reply to the questions contained in the last section will have laid a firm foundation, and will be well prepared for turning his attention to the higher departments of the science. We can therefore confidently recommend this Manual, both to students and to teachers of chemistry, as an excellent syllabus for a practical course of instruction.

The General Glaciation of Jar-Connaught and its Neighbourhood, in the Counties of Galway and Mayo. By G. H. Kinahan, M.R.I.A., Of the Irish Branch of the Geological Survey of the United Kingdom; and M. H. Close, M.R.I.A. Dublin: Hedges, Foster, and Co. 1872.

The mapping of both kinds of the glacial phenomena considered in this pamphlet was commenced seven years ago by Mr. G. H. Kinahan, and was carried on during the course of his work on the Geological Survey. The pamphlet may be said to include a complete view of the glaciation of the district, although many admirable notices and descriptions have appeared from the pen of Prof. King, Messrs. Ormsby and Campbell, and others.

Signal Service U.S. Army: Telegrams and Reports for the Benefit of Commerce and Agriculture. Published by Order of the Secretary of War.

"They do these things better abroad," is as true of meteorology as of many other instances of perhaps more personal moment. A vast number of observations have been shown by Mr. Norman Lockyer to be necessary to the determination of a weather-cycle, and it may be considered probable that the nation first achieving a collection of these data will be the first to make a decisive step in meteorological science. If this hold goods, America certainly appears the country to which the honour will accrue. We have received a copy of these telegrams published during one day by the U.S. Army Signal Service, accompanied by two weather maps. The telegrams give (for seventy-three stations) the height of the barometer, the change in the last eight hours, the temperature and change in the last twenty-four hours, relative humidity, the direction, velocity, and pressure (in the per square foot) of the wind, the amount and direction of upper and lower clouds, the rain-fall, and the state of weather at each station.

These particulars are issued and telegraphed thrice daily, and during the day there are also issued maps of the Continent, showing what has been the state of the weather during the last four-and-twenty hours, and what will probably be the state of the weather during the next twenty-four. Upon the immense importance of such numerous details it is impossible to be too emphatic; a similarly perfect system should be demanded by science from our own Government before it should be too late to reap the full benefit of the labours of English meteorologists. We know that great progress has been made in our own meteorological department, but still we are very far from the advanced ground of our American cousins, who will quickly bear off the palm in this respect, if it be not already gone.

PROGRESS IN SCIENCE.

MINING.

Considering the present exceptional state of the coal-market, it is by no means surprising that public attention should eagerly fasten on the news of any discovery of coal, or of kindred mineral, which may perchance afford a seasonable supply of fuel. Unusual currency has, therefore, been given to certain announcements respecting the recent discovery of coal in different parts of the British Isles; but most of these announcements refer to localities where the existence of mineral fuel has long been known to the geologist, and we must confess that at present there seems no likelihood of any really new centres of coal-mining being established.

Whitecliff Bay, at the eastern extremity of the Isle of Wight, is one of these coal-bearing localities, to which attention has recently been directed. It appears that the gales in the Channel have swept away much of the sand and shingle which usually cover the shore of the bay, and have laid bare what has been described as a seam of coal, from 6 to 7 feet in width, extending in a straight line from the foot of the cliffs down to low-water mark. The tertiary strata in Whitecliff Bay rest in an almost vertical position against the highly-inclined chalk, and, striking directly across the island from east to west, reappear on the opposite side, in the well-known section in Alum Bay. Now it happens that in Alum Bay beds of lignite have long been known to occur in that division of the Middle Bagshot series known as the Bracklesham beds. Prof. Ramsay and Mr. Bristow examined these beds in 1860, and observed that each seam was based upon a stratum of clay resembling the underclay of the coal-measures. Some few years ago similar beds were detected by the Geological Survey in Whitecliff Bay; and the recent discovery resolves itself into a fresh exposure of these Bracklesham coals. It is not, however, likely that these seams will ever prove of any commercial value.

Rumours are afloat of great discoveries of coal and cannel in Sutherlandshire. Yet, as far as can be gathered from authentic sources, it seems that these reputed discoveries refer merely to the coals and shales of the Middle Oolites of Brora, well known to every geologist. No one denies that in certain parts of the world great deposits of Mesozoic coal are extensively and profitably worked; but, bearing in mind the very limited occurrence of such coals in our own islands, it seems doubtful whether their commercial development will ever be remunerative to the British capitalist.

In Elginshire there are not wanting voices to advocate a search for coal, in spite of the adverse geological conditions of the locality. It is true Mr. Judd's admirable researches on Scottish Geology have recently placed beyond doubt the fact that the celebrated Elgin sandstone must be referred to the New Red and not to the Old Red sandstone—a conclusion to which Prof. Huxley's study of its reptilian fossils had previously pointed. Nevertheless this conclusion does not in any way lend itself to the support of those views on the probability of finding coal which have found expression in some of the Scotch papers. For it is surely the height of geological folly to suppose that every bit of New Red must needs have coal-measures beneath it: and, indeed, the highest geological authorities are of opinion that these measures were never deposited within the area of the Elgin sandstone. We are glad to observe that the "Athenæum" has called attention to the folly of this projected enterprise, which has been so staunchly, yet unscientifically, supported by the "Elgin Courant."

Some valuable researches on the conditions under which safety-lamps are truly safe have been conducted by Mr. R. Galloway, partly at the new Laboratories at South Kensington, and partly at the Meteorological Office. Mr. Galloway has already found that if a Davy lamp be burning tranquilly in an explosive atmosphere, the transmission of a sound-wave, produced by a slight explosion of gunpowder, is sufficient to determine the communication of flame from the lamp to the surrounding atmosphere. Hitherto it has been generally assumed that the occurrence of a colliery explosion, after firing a shot, is due to actual communication of flame from the gunpowder to the fire-damp; but Mr. Galloway's experiments show that it is much more likely that the explosion is determined by the noise of the shot being propagated through the galleries of the mine to the safety-lamps. An admirable experiment to illustrate this point was exhibited by Dr. W. Spottiswoode at a recent lecture at the Royal Institution. A lighted Davy lamp was surrounded by streams of coal-gas issuing from a number of jets round the base of the lamp. One extremity of a long tin tube, open at both ends, was placed in connection with the lamp, while a pistol was fired at the other end, a caoutchouc diaphragm being interposed in the tube to prevent the transmission of a direct current of air. The sound-wave, generated by the report, travelled along the tube, and, as soon as it reached the flame, caused ignition of the surrounding atmosphere—the lamp being immediately enveloped in flames.

An improved self-extinguishing safety-lamp, which appears to combine security, simplicity, and strength, has been patented by Mr. Yates, of Duke Street, Westminster. The lower part of the lamp, containing the reservoir of oil, is furnished with a locking-bolt, which bears against some ratchet teeth fixed to the base of the upper portion, or cage of wire-gauze. While the lamp is being screwed into its cage the bolt runs readily over these teeth, but when the two parts are screwed together it is impossible to unscrew them until this bolt has been withdrawn. This is effected by turning a milled head attached to the unlocking screw at the base of the lamp, but the very act of turning this screw causes the wick to be so depressed in its socket that before the lamp can be opened the flame is effectually extinguished. It therefore becomes impossible for the miner to tamper with his lamp without immediate extinction of the light. Nor is there any inducement to open the lamp for lack of light, for unusual brilliancy is obtained by a silver-plated concave reflector fixed behind the light, and a strong well-annealed glass lens secured in a metal frame in front. It is further claimed for the Yates lamp that, though giving a brilliant light, it consumes much less oil than is usually burnt in the ordinary Davy lamp.

A description of the remarkable deposits of Fossiliferous Iron Ore in Southern Pennsylvania, by Prof. B. Silliman, has been published in the "Journal of the Iron and Steel Institute." These deposits occur in a group of Silurian rocks, known locally as the "Surgent Shales"—the equivalent of our Wenlock beds. Three zones of ore are found on three distinct horizons in these shales,—the Levant ore, the Twin beds of fossil ore, and the Hæmatites at the top of the series. By far the most important of these iron ores are the Twin beds of fossil ore,—an ore notable for its purity, its uniformity of structure, and its wide distribution. "It is believed," says Prof. Silliman, "to be, of all deposits of iron ore in the known world, the most extensive and important."

Attention has been called, by Prof. Hull, the Director of the Geological Survey of Ireland, to the brown hæmatite occurring in the Lower Silurian rocks, at several localities in the counties of Longford and Cavan.

A description of the iron ores of Nova Scotia, and the manufacture of Acadian iron, has appeared in the "Mining Journal." It seems likely that the present high price of iron may lead to the development of the iron-producing resources of this colony.

METALLURGY.

Some improvements in effecting the removal of phosphorus from pig-iron, during the process of puddling, have been introduced by Prof. Scheerer, of the Mining Academy of Freiberg, in Saxony. Chloride of calcium and chloride of sodium are mixed in about equal proportions, and the two salts fused together. The fused mixture is run into waterproof paper cases, each holding about 2 lbs., and these cases are introduced into the puddling-furnace one by one, so that the dephosphorising mixture may be thoroughly incorporated with the charge. To ensure satisfactory results, it is recommended to use three times as much of the mixture as the iron contains phosphorus.

According to a method of preparing steel lately proposed by Messrs. F. Bajault and M. Roche, a mixture of cast-iron and powdered hæmatite is smelted, and the product cast in the form of pigs, these pigs being then heated for a considerable time in a furnace of peculiar construction. The rough steel thus obtained may be melted, either in crucibles or in a reverberatory furnace. A sample of steel prepared in this manner yielded, on analysis—Combined carbon, 0·43 per cent; uncombined carbon, 0·8; silicon, 0·13; sulphur and phosphorus, none.

Mr. H. Defty, of Middlesbro'-on-Tees, has patented his trunk-refinery and puddling-furnace. This furnace is provided with an inclined revolving chamber, surrounded at intervals by cast-iron clips which bear upon pulleys on an inclined shaft, rotated by steam-power. The molten iron passes from the smelting-furnace into the chamber, and the lining of this chamber assists in bringing the metal into a malleable state. The metal passes into an oven at the lower end of the chamber, where it is received, with the slag, in a bogie or ingot-mould; whilst the products of combustion, passing from the furnace through the chamber, are utilised in the cupola furnace employed in preparing the metal.

A new system of fettling with oxide of iron, recommended for use in the manufacture of finished iron, has been patented by Mr. T. Greener and Mr. W. Ellis. The mill-furnace in which such fettling is used should have a gradual fall from the fore-plate of from ½ to ¾ of a foot; the refuse from the iron gradually collects in its descent towards the flue, and is there tapped into a bogie. Before use in the puddling-furnace the fettling is broken up in a Blake's crusher.

Although the manufacture of charcoal-iron is not at present carried on to any great extent in France, there are still a few furnaces which treat high-class ores, and produce a charcoal-iron of first quality. As such works usually possess sufficient hydraulic power to keep the machinery in motion, and as the hot-blast is but rarely employed, the only means of utilising the waste gases from these blast-furnaces seems to be their employment in the puddling-furnace or in the refinery. A method of using these gases has been patented by M. de Langdale, and has been described by M. V. de Lespinats in the "Bulletin de la Société de l'Industrie Minérale." The gases are taken off by a common English cup-and-cone, and are then washed and cooled by a shower of water, so that the aqueous vapour present is effectually condensed. The necessary temperature is obtained in the puddling-furnace by using Siemens's regenerators.

Dud Dudley's quaint treatise, entitled *Mettalum Martis*, has been reprinted, by request, in the "Journal of the Iron and Steel Institute" (1872, vol. ii., No. 4). The same number of this journal contains a translation, by Mr. Ernest Bell, of a German paper, "On the Working of Blast-Furnaces with Raw Coal, at Gleiwitz, in Upper Silesia," by Dr. Wedding. There will also be found in this journal an abridged translation of the Report of the Commission appointed by the iron-masters of Belgium to visit this country and examine the working of Danks's rotary puddling-furnace, at Middlesborough.

A new technical journal is devoting itself to the interests of the iron trade. The old-established "Mechanics' Magazine" has arisen in an entirely new shape, and, under the title of "*Iron*," now forms a useful weekly journal dedicated to metallurgy and allied branches of industry.

MINERALOGY.

Considerable interest was excited a short time back by M. Jeremejew's announcement that he had discovered diamonds imbedded in a rare Russian mineral known as Xanthophyllite.* Wishing to verify Jeremejew's observations, Dr. Knop, of Carlsruhe, has been quietly working at the subject, and has recently come to the conclusion that the so-called crystals of diamond are merely angular cavities, suggesting, it is true, the well-known forms in which the diamond is wont to crystallise, but nevertheless destitute of the veriest trace of diamond, or of any other mineral substance. It might, however, be fairly supposed that the cavities, though now empty, originally contained certain crystalline materials which impressed their angular form upon these hollows. Some curious experiments by Knop lead, however, to an opposite conclusion. He obtained thin sections of xanthophyllite, which, when magnified 1500 diameters, appeared to be absolutely destitute of any of these angular cavities: nevertheless, after treating the preparation with sulphuric acid, numerous cavities were recognised exactly similar to those referred in other cases to the presence of diamonds. In other experiments, fine lamellæ of xanthophyllite were carefully examined in all directions under the microscope, and the entire absence of any crystalline impressions thus determined; the object was then touched with a few drops of concentrated sulphuric acid, and heated until white fumes appeared. The preparation, when cooled, was protected with a cover-glass, and placed under the microscope, when it exhibited swarms of beautiful tetrahedral cavities, sharply defined, regularly formed, and arranged in parallel rows. From these and other observations, the author feels justified in concluding that the angular cavities in the Russian xanthophyllite have nothing to do with the presence of diamonds, but owe their origin merely to the corrosive action of acids.

Further "Mineralogical Notices," by Prof. Maskelyne and Dr. Flight, have been published in the "Journal of the Chemical Society." The first portion of the present communication refers to the heterogeneous substances grouped together under the name of *Isopyre*. It appears that these are, for the most part, merely impure forms of opal, associated with other mineral substances. The rare species to which Brooke, many years ago, gave the name of *Percylite*,—an oxychloride of lead and copper, occurring in beautiful blue crystals, belonging to the cubic system,—has hitherto been known only by a single specimen, said to have come from Sonora, in Mexico, and now exhibited in the British Museum. It is, therefore, of interest to learn from Prof. Maskelyne that Percylite has been found among minerals from South Africa. Among other points of interest in this paper, a comparison is suggested between certain minerals from Redruth, in Cornwall, and those recently discovered at Schneeberg, in Saxony, and attention is called to the simultaneous presence of bismuth and uranium in association with arsenic, in minerals from these widely-distant localities.

Another recent contribution to British mineralogy is due to Prof. Church, who has communicated to the Chemical Society some analyses of certain mineral arseniates and phosphates. The minerals examined comprise some transparent crystals of the fluor-apatite known as *asparagus stone* (Werner's *Spargelstein*); the rare species *arseniosiderite*, which occurs in a deposit of manganese-ore at Romanêche, Macon; and the West of England minerals—*Childrenite*, *ehlite*, *tyrolite*, and *wavellite*.

Breithaupt, the venerable mineralogist, though retired from the professorship which he so long held in the Mining Academy of Freiberg, in Saxony, has not rested from his labours. Unable, through loss of sight, either to read or to write, he dictates to his assistant, Herr Frenzel, and has thus been able to contribute to Leonhard and Geinitz's "Jahrbuch" some recent "Mineralogical Notices." Among these notices he gives the full characters of the mineral which he described some time ago as *Nantokite*. This is a chloride of copper from Chili, containing Cu_2Cl_2, which on exposure to the atmosphere readily becomes encrusted with atacamite; and it is suggested that most if not all the atacamite is probably formed from Nantokite. *Winklerite* is the name given by Breithaupt to a new Spanish mineral, of which large quantities are said to have been sold in England as cobalt-ore. From an analysis by Dr. C. Winkler the following unattractive formula has been deduced:—

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* See Quart. Journ. Science, No. XXXII., Oct., 1871, p. 541.

$$8(5CoO.2CO_2+4H_2O)+6(Co_2O_3.H_2O)+8(2CuO.CO_2+H_2O)+ \\ +4(2CaO.As_2O_5+6H_2O).$$

A new mineral-species has been described by Dr. Lasaulx, under the name of *Ardennite*. It is a brown or pale yellow mineral, occurring in fibrous masses without distinct crystalline form. Analysis shows it to be a silicate of alumina and manganese, with small quantities of magnesia, lime, and ferric oxide; but what is especially notable is the presence of 6·17 per cent of vanadic acid. The mineral comes from Ottrez, in the Belgian Ardennes—whence the name.

Ardennite seems to be the same mineral which M. Pisani has lately described under the name of *Dewalquite*, but his analysis gives only 1·8 per cent of vanadic acid.

The rare Scotch mineral described by Brooke, in 1820, as *Lanarkite*, has been recently studied by Pisani. According to Brooke, it is a sulphato-carbonate of lead, partially soluble with effervescence in nitric acid, and leaving a residue of sulphate of lead. Unable to observe this behaviour, Pisani has been led to analyse a typical specimen of Lanarkite from Leadhills, in Lanarkshire. He finds no carbonate of lead, though upwards of 7 per cent of carbonic anhydride is recorded in some of the older analyses. According to Pisani, the mineral known in most collections as Lanarkite is merely a basic carbonate of lead.

We owe to the same energetic mineralogist a recent analysis of the mineral termed by M. Adam *Arite*. This is an amorphous substance, resembling nickeline, and found in a vein in Mont Ar, at the foot of the Pic de Ger, in the Basses-Pyrénées. The analysis leads to the formula $Ni_2(Sb,As)$, and therefore shows that Arite is not a distinct species, but is merely an arsenical variety of the mineral long known as Breithauptite.

M. Pisani has also published an analysis of the New Jersey mineral *Jeffersonite*, from which it appears that this species belongs to the group of pyroxenes.

The third part of Dr. Carl Klein's "Mineralogische Mittheilungen" has been published, but is a purely crystallographic memoir, descriptive of the zinc-blende and anatase of the Binnenthal, in Switzerland.

It is worth recording that Dr. Kenngott has found, in a specimen of basalt from Landeck, in Silesia, some curious enclosures of quartz.

M. Daubrée has presented to the French Academy of Sciences a note, by Mr. L. Smith, describing the mass of meteoric iron which fell, in 1862, at Victoria West, Cape Colony. The iron yielded on analysis—Iron, 88·83; nickel, 10·14; cobalt, 0·53; phosphorus, 0·28; and small quantities of copper.

The same communication contains some remarks on the mineral *Enstatite*, a silicate of magnesia originally described by Prof. Shepard as *Chladnite*.

Some researches, by M. Pisani, on the native amalgams of silver occurring at Kongsberg, in Norway, show that two distinct amalgams are found,—the one containing Ag_6Hg, and therefore identical with *Arquerite*; and the other containing only 4·9 per cent of mercury, corresponding to the formula $Ag_{18}Hg$. Should the latter prove to be a definite species it is to be called *Kongsbergite*.

A paper "On the Composition of some Zeolites" has been read before the Glasgow Philosophical Society, by Mr. J. Wallace Young. The paper contains analyses of Scotch specimens of analcime, thomsonite, natrolite, and stilbite,—the alkalies in which have been determined by Lawrence Smith's method.

The little green pebbles commonly sold to tourists in Iona have been analysed by Mr. E. C. C. Stanford, and found to be a variety of serpentine, notable for containing manganese.

ENGINEERING—CIVIL AND MECHANICAL.

Guns.—The first 35-ton gun, known by the name of the "Woolwich Infant," has recently formed the subject of a report, as to the state of its interior, by the Inspector of Ordnance. After thirty-eight horizontal discharges, after its bore had been enlarged to 12 inches, the interior had sustained injuries, caused by four cracks, four fissures, and some deep roughnesses: two of the cracks were on the lower side of the bore, and all the other injuries on the upper side, their centres coinciding with the point where the front studs of the shot-hammer and the rear studs come into driving bearing. The gun is being rebuilt at a cost of about £700 or £800.

On the 9th of January last Commander Dawson, R.N., read a paper, at the Royal United Service Institution, on the "Powder-Pressures in the First 35-ton Gun," illustrated by diagrams showing the state of its interior on leaving and on returning to the gun-factories, and by corresponding diagrams of some other disabled Woolwich guns. After thirty-five discharges from the 11·6-inch bore, this gun was reduced by boring it up to 12-inch calibre, with a corresponding reduction of the pressures. But after thirty-eight horizontal discharges the 12-inch bore was so injured by the projectiles as to necessitate the rebuilding of the gun. The table of pressures shows that when the 12-inch bore was fresh from the factory, 110-lb. W. A. P. charges gave very regular maximum pressures of 20·1 tons; but when the 12-inch bore had sustained thirty-four to thirty-eight discharges the registers were very irregular, and averaged 31·3 tons. Similarly, the first 115-lb. W. A. P. charges, in the 12-inch bore, gave very regular mean maximum pressures of 22·5 tons, but subsequently increased to 44·5 tons; and the 120-lb. charges began at 20 tons and increased to 66 tons. The whole of the injuries in the bore of the gun are, however, recorded in a certain short part of the bore outside the area of maximum pressures, and precisely where the oblique movement of the axis of the projectile about its studs would have its greatest force. The same misdirection of mechanical forces was shown to be in operation in other guns similarly rifled, tending to impede the free exit of the shot, to injure the projectile and the guns, and to diminish the velocities and striking force, whilst giving rise to accumulation of gases and elevation of pressures in the bore.

Shells.—A series of experiments have recently been carried out in Austria in order to test the relative merits of steel and chilled iron shells. These trials appear to have been very exhaustive, and the results confirm what had previously been arrived at, namely, that the former are superior for naval purposes. The steel projectiles were found to pierce the shield with a considerable excess of force without breaking up, whilst the chilled shells only penetrate to the second plate and break up. The effect of the live shells also accords with this; the steel shells explode in the wood backing, and their fragments are hurled behind the target. The chilled shells burst in front of the second plate, and thus virtually produce no effect, being kept by the side far from the interior of the ship.

Dynamite.—A very interesting series of experiments have recently been carried out with this powerful explosive, among the sand-hills of Ardeer, on the coast of Ayrshire, where the British Dynamite Company have erected their factory. These experiments were carried out in order to satisfy the traffic-managers of the Scotch railways of the almost total immunity from danger that is displayed by this valuable material under all conditions of carriage. They were conducted under the superintendence of Mr. A. Nobel, the patentee and technical director of the Company. The results were most conclusive and satisfactory, proving the dynamite to be perfectly harmless under mere percussion, or when subjected to flame, but capable of exerting a most powerful effect when exploded in the usual manner with a Bickford fuse.

The mode of using dynamite is to make it into cartridges, and a percussion-cap very similar to an ordinary gun-cap is fixed to the end of the fuse. The cartridge having been opened at one end, the cap is pressed into the dynamite, and secured there by ordinary twine. When used for mining purposes, the cartridge having been placed in the bore hole, and damped with water or sand, the fuse ignites the cap, and the explosion of the cap explodes the dynamite. It has been proved by experiments that a cartridge containing 3 ounces of dynamite has as much disruptive effect as 1 lb. of powder.

Railways.—The Institution of Civil Engineers has been occupied during the whole of six or seven evenings with the discussion of a paper by Mr. W. T. Thornton, of the Public Works Department, India Office, on "The Relative Advantages of the 5 ft. 6 in. Gauge and of the Mètre Gauge for the State Railways of India, particularly for those of the Punjab." The author, in his paper, after referring to estimates for narrow-gauge lines by Mr. Hawkshaw and Mr. Fowler, drew an average between the results of the two estimates, and thus attempted to prove that the saving to be effected by the introduction of narrow-gauge lines into India would not be less than £1000 per mile, which, for the 10,000 miles of State railways already in contemplation, would show a total saving of not less than ten millions sterling in their construction. And it was stated that belief in its superior economy was the one solitary reason why the Indian Government had adopted a narrow gauge for its State railways. After going into a lengthened discussion, having reference more particularly to the Punjab railways, the case for the Government of India was summed up thus:—That by making the Punjab lines on the metre gauge it would save £530,000, at the lowest computation. To have adopted a light standard, instead of a metre gauge, would have occasioned a waste of a like amount, against which there would not have been the smallest strategical set-off, nor any other compensation of any kind, except a slightly increased commercial convenience, not exceeding in capitalised value £17,000 at the outside.

Soudan Railway.—Perhaps one of the most important lines of railway communication now in course of construction is the Soudan Railway, running up the Valley of the Nile, in Egypt, and destined not only to open up the rich country traversed by that river, but eventually it will also doubtless form a very important rival to the Suez Canal route to India, as it will, when completed, shorten the length of the journey by three days. In consequence of the hard rocky nature of the ground, in many parts, the proposal to canalise the Nile so as to form a continuous water-communication past the great cataracts, as was proposed by Mr. Hawkshaw in 1865, will not be adopted. According to the plan proposed by Mr. Fowler, and now under construction, the first cataract will be passed by a ship-incline of 2 miles in length, to be worked by hydraulic power; and at the second, or great cataract, a line of railway—560 miles in length—will open up communication to the Soudan country, and this will eventually be extended to Massowah, on the Red Sea, a further distance of 430 miles. This new route will be altogether 1900 miles in length. Commencing at Alexandria, on the Mediterranean, the existing railways terminating at Roda will cover 310 miles of the distance. At Roda the passengers will be transferred to light and swift steamboats, and for 600 miles southwards the Nile will form the highway for inland traffic. In this distance the first cataract has to be passed, at which there is a difference in level of about 12·5 feet at high, and 15 feet at low Nile. This, as we have said, is to be passed by the construction of a ship-incline, nearly 2 miles in length, on the right bank of the river, commencing at the bottom of the cataract between the island of Sehayl and the river-bank, and terminating on the higher level in the harbour of Shælall, north of the celebrated island of Philæ. Rails will be laid on the incline, and suitable carriages constructed to run upon them. The vessel to be raised or lowered will be floated upon these carriages or cradles, the ship and carriage being then drawn over the incline by hydraulic engines driven by water, at high pressure, pumped into huge accumulators, at the summit of the incline, by a pair of large water-wheels placed upon pontoons and moored in one of the rapids of the cataract. A speed of from 4 to 7 miles an hour will thus be imparted to the vessel, according to the height of the Nile and weight of the vessel. Thence the river communication will extend to Wady Halfa, the commencement of the Soudan Railway. A transference from steamboats to railway will take place at this point, and the 560 miles of the Soudan Railway will extend to Skendy. From Skendy to Massowah, the port on the Red Sea where the sea-passage will be again resumed, is 430 miles, which will be accomplished by an extension of the Soudan Railway. The gauge fixed on for the railway is 3 feet 6 inches.

Rail Economy.—In December last a paper, by C. P. Sandberg, was read before the American Society of Civil Engineers, in New York, upon "Rail Economy," in which—under the three heads Iron Rails, Steel Rails, and Traffic Capacity—the author dealt with the saving that might be effected in the item of railway cost. It was remarked that the late increased expense of iron added to the cost of railway construction, and tended to reduce the quality of rails; that Welsh rails were now often inferior in quality, but in the Cleveland district rail-making had greatly improved, chiefly by the increased application of fettling in the puddling-furnace. No late improvement, it was observed, promised so much to perfect iron rail-making as mechanical puddling, which now seemed to be an entire success. The demand for steel rails has become so great that they can now hardly be obtained at any price, whilst the supply is also limited by the lack of ore free from sulphur and phosphorus. The Siemens-Martin process of steel-making is declared to be superior to the Bessemer process, as it requires a less pure ore, but it has thus far proceeded so little that it can hardly be called a source of supply in the great market. Usually a steel rail will carry one-fifth more dead load than an iron one; hence, for the same traffic, the steel rail, in comparison with the iron, should not be reduced in weight more than 20 per cent. The weight passed over good iron rails, before rejection, has been found to average 10,000,000 tons, which may be taken to represent the life of extra iron rails, and six times that the life of good 56-lb. steel rails. On the London and North-Western line steel rails have lasted twenty times as long as iron; and on the Metropolitan Railway, with the greatest traffic in the world, where iron would not have lasted six months, steel will stand from three to four years. Prof. Rankine says the weight of the rails per yard in length should equal fifteen times the greatest load on the locomotive-drivers in tons. Perdonnet, in France, takes twelve in place of fifteen. The author of the paper, by adopting a section which permits a fish-joint stronger than the others in general use to be made, takes ten and less; thus for a 60-lb. rail the weight on drivers is put at 6½ tons. Fish-plates of steel will enable rails to carry from 15 to 20 per cent greater load than if iron were used of the same section.

GEOLOGY.

Obituary.—The Rev. Adam Sedgwick.—Geological science has expanded so much during the past fifty years that it is difficult for any one man to be master of all the subjects it embraces. Sir Charles Lyell has expressed the difficulty he has felt from year to year in keeping up with its progress, and no man has done more to further the advancement of geology than he, by presenting the principles and general results of the science before the public. We have very few of those veteran geologists left who connect, as it were, the early history of geology with its present advanced state, who have contributed most largely to lay the foundations (which are lasting monuments to their honour) to which the geologists of the present day are adding detailed work—and there is plenty of that to be done.

The Rev. Adam Sedgwick, who died on the 27th of January last, at the advanced age of 87, was one of those veterans who helped to lay the foundations of geological science, and who is therefore intimately connected with its progress. Although for some years past he took no very active part in the furthering of geology, he yet remained until death at his post of Professor of Geology in the University of Cambridge, which post he had held since the year 1818, when he succeeded Professor Hailstone.

At this period little was known in England of geological science, but a general notion prevailed, agreeing closely with the theory of old Dr. John Woodward, who founded the chair, that all fossils were the result of a universal deluge which had once swept over the whole earth, and to the agency of which all the strata owed their origin.

Professor Sedgwick directed some of his earliest inquiries to the structure of Devon and Cornwall, in which counties the relations of the rocks present problems of great difficulty—even now much discussion takes place in regard to them, as was pointed out in the last number of the "Quarterly Journal of Science." Professor Sedgwick, sometimes accompanied by Sir Roderick Murchison, examined the district in great detail, and determined, if not the true age of the beds, their true succession.

Professor Sedgwick devoted his attention at times to the Continent, and explained the geological structure of the Alps and Rhenish provinces. In the "Geological Magazine" for April, 1870, there was a biographical notice and a portrait of this eminent geologist: in the former was a list of forty-four papers contributed by him, a few in conjunction with Sir R. Murchison or Mr. W. Peile—all contributions to geological science. Among these we may mention his Memoirs on the Magnesian Limestone of the North of England; on the Trap Rocks of Durham and Cumberland; on the Fossiliferous Strata of the North of Scotland, and on the Isle of Arran; on the Mountains of Cumberland and North Wales; and his Essays on Slaty Cleavage.

These show the extent of country examined by Professor Sedgwick, and the many subjects he was master of.

No member of his University has contributed in a higher degree than he to elevate its character as a school of the natural sciences, and many of our leading geologists owe their first geological lessons to Sedgwick, who as a lecturer was clear, earnest, and philosophical, full of energy, and even to the last vigorous, and, when his health permitted, cheerful and full of humourous anecdote.

To Professor Sedgwick also the University is indebted for much care and liberality in providing for the now large collections of the Geological Museum, the nucleus of which was Dr. Woodward's own small cabinet.

It is some satisfaction to learn that the post of professor of geology in the University of Cambridge left vacant by the decease of the venerable Sedgwick, has been filled by a distinguished pupil of his—Mr. T. McKenny Hughes, M.A., F.S.A., F.G.S., of the Geological Survey of England and Wales. Mr. Hughes has done much detailed field-work on the geological survey in Kent, Hertfordshire, and in the Lake District. He has written portions of the Survey Memoirs illustrating the geology of the Lake District, and has also contributed largely to Mr. Whitaker's Memoir on the Geology of the London Basin. The "Quarterly Journal of the Geological Society," and the "Geological Magazine," contain papers by Mr. Hughes, and he is not only known as a clear writer, but as a ready and clear lecturer.

Geological Awards.—The awards of the Geological Society of London may be looked upon as the highest honours conferred upon geologists in this country, and they are intimately connected with the progress of the science, being either the reward of a life's devotion to its advancement, or a stimulus to one in early life to continue researches which have materially assisted the progress of geology. At the Anniversary Meeting of the Society, held in February last, the president, the Duke of Argyll, presented the Wollaston Gold Medal to Sir Philip Egerton, Bart., F.R.S., &c., for the services he has rendered to geology, during a period extending over forty years, in the special attention he has bestowed on the structure and affinities of fossil fishes and reptiles. The balance of the proceeds of the Wollaston Donation Fund was awarded to Mr. J. W. Judd, F.G.S., in recognition of his valuable researches in the Neocomian and Jurassic rocks of England, and in the Oolitic rocks of the west coast of Scotland. The Murchison Medal, the first award made under and in fulfilment of the will of the late Sir Roderick Murchison, was handed to Mr. William Davis, of the British Museum, in recognition of the services he has rendered to palæontology, in the skill and knowledge he has displayed in the reconstruction of extinct forms of life; and the balance of the Murchison Fund was awarded to Prof. Oswald Heer, of Zurich, for his researches in fossil botany and entomology, and particularly for the light he has thrown upon the Miocene Flora.

Stratigraphical Geology.—Attention has been directed, particularly on the Continent, to deposits which fill up gaps in the table of strata. England is no longer considered as forming an epitome of the geology of the world, and yet gaps are being filled up in it, rather than new unconformities made out. On the Continent, the Tithonic stage of Stramberg forms in places a gradual passage between the Neocomian and Jurassic strata, which elsewhere, as in Northern Germany, Yorkshire, and Lincolnshire, are uncomformable.

Some discussion has taken place in regard to the Punfield formation, named by Mr. Judd. This he regarded as Neocomian, though still closely connected with the Wealden, and, in fact, forming a transitional series of beds between the two, though absolutely belonging to neither, and therefore worthy of a distinct name. Mr. Meyer, who recently read a paper on the subject before the Geological Society of London, repudiates the distinctive name, and includes the Punfield beds in the lower Greensand.

The Midford sands, so called by Prof. Phillips, occupy an intermediate position between the Inferior Oolite and the Upper Lias clay, but for a long time their true position was obscured by the appellation of either Upper Lias sands, or sands of the Inferior Oolite, according to the formation to which the writer inclined to consider them as more closely related. The term Midford sands, therefore, removes a good deal of misunderstanding, and its adoption will cause the neutral position of the beds to be better recognised.

In the same way the discussions as to whether the Rhætic or Penarth beds of England belonged more closely to the Lias or the Keuper are rendered needless, when both their stratigraphical and palæontological features are taken into account. It is now known that they present a gradual passage between the two, and although but a feeble representative of the beds developed on the Continent, they are yet a complete series in our country, and, as such, link the Keuper and Oolitic rocks in one conformable series.

It is becoming more evident that the sequence of beds which holds good in one place requires some modification in another. Like sedimentary conditions certainly did not always prevail over very large areas, while the organic remains will vary in a measure according to the different physical conditions which prevailed. Thus our Oolitic system varies greatly in its extension from Yorkshire and Lincolnshire, through Northamptonshire and Oxfordshire, to Gloucestershire, Somersetshire, and Dorsetshire. The divisions of the Northamptonshire Oolites have received a great deal of attention recently from Mr. S. Sharp and Mr. J. W. Judd, and the former geologist has this year given a second paper to the Geological Society of London on the subject. He showed that the series of beds were as follows:—

Great Oolite	Clay Limestone Upper Esturian Clays.	
Inferior Oolite	Lincolnshire Limestone (present only as a thin band in the north-east portion of the district.)	
	Lower Estuarine beds Feruginous Beds	Northampton Sand.
Upper Lias	Clay.	

He considered that the great Oolite clay represented the Forest Marble and the Bradford clay of the West of England; that the Great Oolite limestone was nearly equivalent to the Great Oolite of Bath and the Cotteswolds; that the Upper Estuarine clays were identical with the Stonefield slate of Oxfordshire; that the Lincolnshire limestone was nearly synchronous with the grey limestone of Yorkshire (Inferior Oolite), and probably with the lower portion of the *Am. Humphriesianus* zone of the West of England, but extending a little below this zone; that the Lower Estuarine answered to the Lower Plant Shale of Yorkshire, but had no representative in the west; that the upper portion of the ferruginous beds of the Northampton sand was nearly upon the same horizon as the Glaizedale and Dogger beds of Yorkshire and the *Am. Murchisonæ* zone of the west; and that the lower portion of the Northampton sand was represented by the *Am. opalinus* zone and the Midford sand.

Palæontology.—Mr. S. H. Scudder has recently described a new fossil butterfly (*Satyrites Reynessii*), from a tertiary deposit at Aix, in Provence. The fossil is a natural imprint, and its state of preservation shows that it had undergone great maceration in quiet water before being covered up by the deposits which have preserved its most essential features. The insect is placed on its side, with the wings elevated one against the other, the legs spread out as if it were suspended, the spiral proboscis unrolled, and the antennæ lowered in the same direction as the legs. The nearest living representative of this fossil butterfly are natives of India.

Mr. A. G. Butler has described, in the "Geological Magazine" for January, a new fossil butterfly (*Palæontina Oolitica*) belonging to the family *Nymphalidæ*, This insect belongs to a group completely tropical, its nearest allies being tropical American genera. It is interesting as belonging to the highest family of butterflies, and to a sub-family intermediate in character between two others—namely, the *Satyrinæ* and *Nymphalinæ*, but it is especially interesting as being the oldest fossil butterfly yet discovered.

Prof. O. C. Marsh, who has added largely to our knowledge of the fossil vertebrata of North America, announces the discovery of fossil quadrumana in the Eocene strata of Wyoming. These remains closely resemble, especially in many of the larger bones, some of the Lemurs, while the anterior part of the lower jaws is similar to that of the marmozets. The teeth are more numerous than in any known quadrumana.

Prof. Marsh also describes a new carnivore from the tertiary strata of Wyoming, named *Oreocyon latidens*. The remains indicate an animal about as large as a lion: the canine and premolar teeth of the lower jaw somewhat resemble those in the hyæna, but there were only two incisors in each ramus.

One of the most interesting discoveries of recent years has also just been made known by Prof. Marsh. It belongs to a new sub-class of fossil birds (*Odontornithes*). The remains were discovered in the Upper Cretaceous Shale of Kansas. The type species of this group is called *Ichthyornis dispar*: it has well-developed teeth in both jaws, very numerous, too, and implanted in distinct sockets. The possession of teeth, and also of bi-concave vertebræ, imply that these remains cannot be placed in the present group of birds, and hence a new sub-class has been formed. The bird was no larger than a pigeon; it was carnivorous, and probably aquatic. The fortunate discovery of these interesting remains is an important gain to palæontology, and does much to break down the old distinctions between birds and reptiles, which the *Archæopteryx* has so materially diminished.

Prof. H. A. Nicholson has described a new genus of Tubicolar Annelides, (*Conchicolites*) for some forms found growing upon the shells of *Orthocerata* in the Lower Silurian rocks of the north of England. The tubes in this genus agree with those of the modern *Serpulæ* in being calcareous, and they differ altogether from those of the extinct genus *Cornulites* in being altogether destitute of any cellular structure.

The affinities of *Calceola sandalina*, one of the most problematic of fossils, have been lately discussed by the Rev. T. R. R. Stebbing. It has been referred to the Conchifera, Rudistes, and Brachiopoda, but Mr. Stebbing agrees with Suess and Lindström in placing the genus among the Zoantharia rugosa. He refrains from giving the convenient name of coral to the *Calceola*, because Lindström, following Agassiz, gives reasons of some weight for separating the Rugosa from the true polypes, and conjectures them to be allied rather to the Hydrozoa than to the Anthozoa.

Dr. Anton Fric has described a new crustacean from the Polirschiefer, near Bilin, in Bohemia, which he names *Palæmon exul*. The discovery is remarkable, on account of its being a marine crustacean in a fresh-water deposit.

Mr. H. Hicks has described a number of new species of fossils from the Tremadoc rocks of St. David's, South Wales, which prove that these rocks are nearly allied to the lower part of the Tremadoc rocks of North Wales. The discovery of a number of well-marked species of *Lamelli branchiata* in beds of an earlier date than those in which their presence had previously been known is of great interest.

Mr. Henry Woodward has described a new genus of shore-crabs, *Litoricola*, from the Lower Eocene deposits at Portsmouth; and a new trilobite, *Encrinurus cristagalli*, from the Cape of Good Hope.

From a letter we have received from Prof. Piazzi Smyth, the Astronomer-Royal for Scotland, we find that there has been a good deal of exploring work going on lately at the Great Pyramid. An explorer has been found in Mr. Waynman Dixon, a young engineer of Newcastle-on-Tyne, who has been in Egypt for nearly a year and a half, building an iron bridge across the Nile at Boolak, opposite to the Pyramids. He went out well read up in the new scientific theory of the Great Pyramid, and most earnest to promote its development, and has utilised all his spare time towards that end—latterly indeed, after his bridge was finished, going out to the Pyramid hill, living in the tombs there to be close to the spot, and attended by our old Arab servants as well as some of his own English workmen. His elder brother, Mr. John Dixon, of Cannon Street, London, occasionally went out to help, and some companionship was afforded during part of the time by Dr. Grant, an English physician in Cairo, but the soul of the whole movement was Mr. Waynman Dixon himself. In company with his head carpenter, "Jim Grundy," Mr. W. Dixon has taken casts of critical portions of the coffer, also of the "boss" on the granite leaf, has observed thermometers extensively, and taken several important re-measurings. He has also been boring away in divers places, *hoping* to find another chamber; but neither chamber nor passage has he yet met with, though in the Queen's chamber he has discovered the inner ends of two small channels like those in the King's chamber. They are rectangular, 9″ x 8″ nearly, go back horizontally about 7 feet, and then rise at an angle of about 32°, and go no one yet knows where. These channels had not been recognised before, as this outcrop into the Queen's chamber had been neatly filled up with a thin plate of white stone, looking superficially like the rock of the walls. One of them is in the north wall, and the other in the south. Inside them were found squeezed out flakes of white mortar (since then analysed by Dr. Wallace, of Glasgow, and found to be not carbonate, but sulphate, of lime), an ordinary "miva" stone-ball weight of the ordinary old "profane" Egyptians, a little bronze sort of grapnel hook, and a little staff of trimmed cedar-like wood a few inches long, but nearly perished. These channels it is proposed to call Dixon's channels. Outside the Pyramid, Mr. W. Dixon has discovered the finest specimen of a loose casing stone of the Great Pyramid known to exist; and he has also, in company with Dr. Grant, made a grand expedition into the Libyan Desert to examine the supposed Pyramid there, hitherto called Dr. Luder's Pyramid. It turned out to be no artificial structure at all, but a natural hill of a conical shape, and near it were abundant remains of silicified tree-trunks lying here and there, with petrified shells and jasper pebbles. From what has been done we may gather—

(*a*). The casing stone fragment has five worked surfaces, and two of them being ends, we can measure for the first time the *length* of a casing stone of the Great Pyramid as well as its angle; and what is its length? Twenty-five inches and a fraction, or the sacred and scientific cubit,—not of the profane and idolatrous Egyptians, but of Noah, the Hebrews, and the anciently *concealed* parts of the Great Pyramid.

(*b*). Two measures were made in the extreme passage by Mr. W. Dixon, Dr. Grant touching a line on either wall, supposed to have been drawn for an important purpose by the very architect of the primeval monument himself, and requiring, according to the scientific theory, to show a distance of 2170 inches from a crucial part of the interior. The result along the east wall shows 2170·5, and along the west wall 2170·4, when used in conjunction with Prof. Smyth's measures in "Life and Work," printed long before that theoretical conclusion had been thought of.

(*c*). Mr. John Dixon sent an account and drawings of the findings in Dixon's channels to the "Graphic" newspaper. So far so well. But he also sent drawings of well-known parts of the Pyramid,—hand drawings,—and very accurate ones too, of parts of the Pyramid already *photographed*; and thereby he destroyed knowledge, and retarded the development of the public mind.

(*d*). Prof. Smyth weighed the grey granite ball, and found it 8320 grs.; and therefore declared it to be a profane Egyptian miva belonging to some of the hodmen about the Pyramid; Sir G. Wilkinson having already published the weight of the Egyptian miva at 8303 grs. But he advised Mr. W. Dixon to have it authoritatively weighed by the "Warden of the Standards."

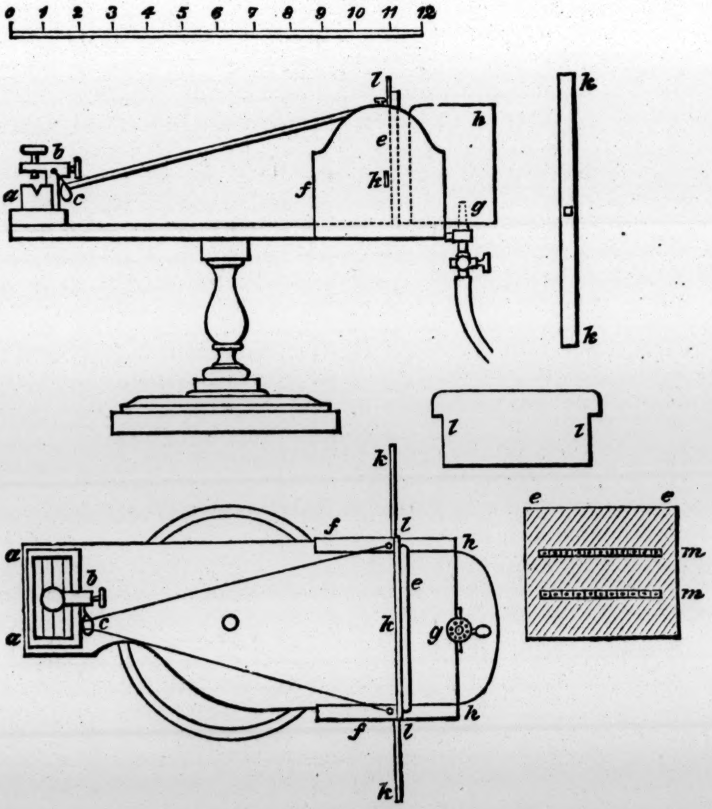
(*e*). The Warden of the Standards did so, made the weight 0·03 gr. different from Prof. Smyth's, and then wrote a letter to "Nature," December 26, describing the whole affair on one side, giving a wandering anachronical conclusion of his own, that the ball *may* have been an Egyptian *miva*, without saying a word of Prof. Smyth's conclusion; and then, worse still, declaring that Sir. H. Jarvis's pamphlet in 1869 was the *latest* and most satisfactory account of the length of the Great Pyramid's box-side, and advocating both his (Sir H. J.'s) mistaken reading of Herodotus and his garbled version of the result of direct measures.

(*f*). Two letters have been sent by independent parties to "Nature," pointing out the errors of the Warden of the Standards, but its editor has refused them both insertion; and, consequently, a third party has sent off a letter to "Les Mondes" in Paris.

LIGHT.

Mr. H. R. Procter has described to the Newcastle Chemical Society a glass reading-scale for direct vision spectroscopes. The apparatus consists of a mahogany frame, of ½-in. planed boards, for holding Browning's direct vision pocket spectroscope and reading-scale. *a a*, wood block with V-groove for

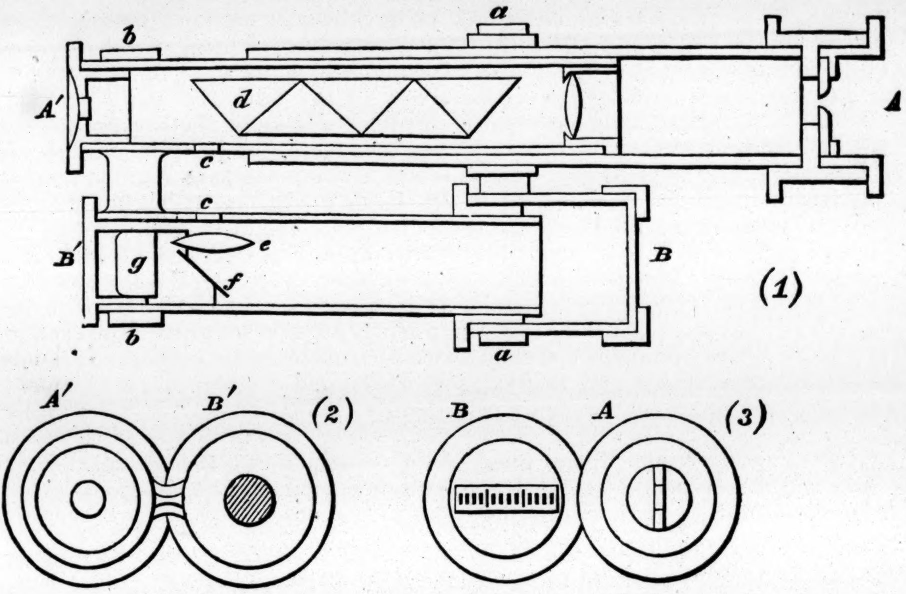
FIG. 1.



holding the spectroscope; *b b*, brass clamp and screws for fixing it in its position, and for holding the wire ring, *c c*, to stretch the wire supports, *d d*, of a black curtain for keeping out stray light from the side-hole of the spectroscope; *e e*, photographed glass millimetre, or other scale of equal parts, sliding between two upright mahogany cheeks, *f f*, and lighted from behind by the gas-jet, *g g*; *h h*, a tin screen to shade the flame; *e e*, the dark-glass scale with transparent graduations at *m m*, seen in the spectroscope above and below the spectrum; the upper one can be shut off by a black card screen, *l l*, slipped down in front of the glass plate; and all but three scale-divisions of the lower one, at any point, can be stopped out by a card slider, *k k*, slipping through slits in the two side-cheeks of the frame, to confine the vision to the graduations closest to any line. The glass plate is backed behind by a piece of oiled paper gummed by its edges to the photographic plate. Prof. Herschel stated that he has also made an addition to Browning's pocket microscope. He put the thinnest possible film of mica on the glass plate which fitted the eye, and he held it in place with a small india-rubber ring. That film of mica, by inequalities of its thickness, or other refracting properties which it possesses, eclipses certain rays of particular refrangibility, and, according to the thickness, it will eclipse more or fewer bands in the whole range of the spectroscope; so that, looking through it, you get the spectroscope divided into compartments by dark bands. As these bands are not easily seen, he thought he would bore a hole in the side of the spectroscope, and get a scale reflected in its prisms; and if this scale was to be a pocket one, he must, in this hole, which was bored through, put a small lens, and fix the scale close in front of that lens, for use as a scale of reference. The use of a spectroscope resolves itself into placing every line according to its scale position on any arbitrary scale, but as far as possible on the scale of what is called the natural standard scale of wave-lengths. If the position of every line which is mapped can be given in wave-length, its description is then intelligible to everybody. The scale fitted to this spectroscope, of uniform intervals, would enable spectra to be recognised; but being a new instrument its indications first require to be reduced to some well-known standard. The simplest way of recording them would be by wave-lengths. The positions of successive lines, as seen in the common spectroscope, are not in the simple proportions of their wave-lengths; the blue lines are more spread out than corresponds to the differences of their wave-lengths, and the red lines are nearer together, while the wave-lengths in the latter part of the spectrum are far apart; and therefore the question was how to pass from a uniform scale used in a spectroscope, in the manner shown by Mr. Procter, to the scale of wave-lengths. He had quite recently found that a uniform scale, used with any spectroscope, like that divided in Mr. Procter's instrument into millimetres, is approximately proportional to a scale of inverse fourth-powers of the wave-length. If, for example, we take the readings of the sodium line, and of any other known line of the spectrum, on a scale of equal parts, and replace them and all the other readings of the scale in proportion by the inverse fourth-powers of the wave-length, we would then find that we can pass from the uniform scale to the wave-length by taking the inverse (or reciprocal) of the readings so replaced, and taking the fourth-root of that reciprocal to obtain the wave-length. It so nearly was the case in all prisms of ordinary dispersion, as to present an almost accurate means of passing without trouble from the uniform scale of the spectroscope to the wave-lengths; and although it is not quite true of the whole range of the spectroscope, yet if used between the short interval of two neighbouring lines to find the wave-length of an intermediate line, it will give the wave-length of that line directly from the milimetre scale.

The following is a description of Procter's direct-vision micrometer scale for pocket spectroscopes. A A′, small direct-vision spectroscope. B B′, a parallel brass tube (first slide of a miniature toy telescope) braced to the tube and draw-tube of the spectroscope by the double rings of bent copper plate, *a a*, *b b* (seen also in Fig. 2). The ring, *a*, slides on the main tube of the spectroscope by a collar of soft leather interposed between them. *cc*, small holes about one-eighth inch diameter drilled opposite to each other in the brass tubes for viewing a magnified image of the reading scale, B, reflected in the prism face, *d*. *e f*, a lens of short focus, and thin silvered glass diagonal mirror, cemented on a cork, *g*, in the stopped eye-end of the telescope tube, for obtaining a magnified view of the glass reading scale at B. The latter is marked in transparent lines on an opaque ground, as shown in Fig. 3, and is illuminated by the same direct light as that of which the spectrum is observed.

FIG. 2.



We are indebted to Mr. R. C. Johnson for an account of a curious physical phenomenon witnessed at Ziza—a lunar dew-bow. Ziza is a ruined city which is situated about 20 miles E. of the northern part of the Dead Sea on the table-land of Moab, and is about 3000 feet above the sea-level. The Moabite Expedition was camping there on the night of the 24th February, 1872, close to a large reservoir. Mr. Johnson says, "It was full moon (at 11 a.m. of the same day), and having seen ducks come down to the reservoir, we turned out about 8 p.m. to lie in wait for them against the sloping banks of the reservoir. An *exceedingly* heavy dew had fallen, and I noticed when walking with my back towards the moon that I was preceded by a faint circular halo (extent of circle about ⅓ of circumference); the origin of which at first was rather puzzling. On attentively considering the position of my eye and the halo with regard to the moon, I found that it was exactly at the angle required for an inverted rain-bow, and that it must really be a dew-bow. It seemed brighter than a lunar rain-bow, which I have once beheld. Dr. Tristram also noticed it after his attention had been called to it. I may also state that a similar thing was seen in sunlight when a very fine dew was thickly spread upon some large webs made by caterpillars in the same country. This was looked out for after having previously seen the lunar dew-bow.

An instrument invented in Germany for testing colour-blindness consists of a rotating apparatus, which moves a disc whose centre is a circle, one half black and the other white. Outside of this is a ring half red and half green, then another ring of violet and red, then the outside ring of violet and green. When rapidly rotated, the centre appears to be coloured grey, that is black and white mixed. To a green-blind person, the middle ring will appear grey, that being a result to him of a mixture of violet and red. The outer ring will appear grey to a red-blind person, and the inner one grey to a violet-blind.

MICROSCOPY.—A new form of pocket microscope has been contrived by Prof. G. T. Brown of the Royal Veterinary College. In general construction it somewhat resembles the well-known clinical microscope of Dr. Beale, but is very much smaller, the extreme length being only 3 inches, and packed in its case with two slides and some thin glass measures 3¼ inches in length by 1 inch wide, and 1⅛ inches in depth. A case double in width will hold, in addition, an extra eye-piece, two objectives in boxes, a glass tube, and two dissecting needles. The short body necessarily involves a loss of magnifying power; this is, however, met by using a very deep eye-piece, the instrument with an E eye-piece giving the same power as an A eye-piece with a 10-inch body. The fine adjustment is made, as in Dr. Beale's instrument, by sliding the draw-tube containing the eye-piece, a mode of adjustment characterised by its extreme sensitiveness. It is capable of working with powers as high as an immersion twelfth. The instrument has been especially designed for observations in veterinary practice, where it is necessary to have the microscope ready for use at any place: for this purpose it is especially suited, as packed in its case, it is less in dimensions than any case of surgical instruments. It will no doubt prove very useful to field naturalists, and often prevent worthless gatherings being brought home by permitting examinations to be made on the spot. The only disadvantage of the microscope is, that owing to its small size, the objectives and slips of glass are of corresponding dimensions. This prevents the utilisation of the observer's stock of objectives, and also the examination of objects mounted on the usual 3 × 1 slides.

At the suggestion of Dr. Pigott, a micrometer scale has been ruled by Mr. Ackland on the flat side of a plano-convex lens of very long focus; this is inserted into the diaphragm of the eye-piece. The definition of the object is less impaired than when the old form, having several plane glass surfaces, is used. It is also easier to make with accuracy, as there is always more or less difficulty in working perfectly true parallel surfaces. It will doubtless prove of equal value for dividing the field of the microscope into a number of squares for the purpose of making drawings, a favourite method with some observers, and to whom better definition than with the original plane disc will be a welcome improvement.

Some guide to beginners in the use of the micro-spectroscope has long been a desideratum; this want is likely to be supplied. An introductory work on the subject, with numerous figures of absorption-spectra, lithographed by the author, is reported to be in progress. Work in this department has been much hindered by the want of such help, many instruments being almost useless to their owners for lack of some practical hints as to their employment. The subject is one which has a future before it; little or nothing is at present understood as to cause of the remarkable phenomena of the absorption-spectrum, and there is plenty of work, with the probability of valuable discoveries, for those who apply themselves to such researches.

Mr. T. Johnston English has brought before the Quekett Microscopical Club a new apparatus for injecting animal tissues for microscopical purposes. The instrument consists of a Woulfe's bottle with three necks. No. 1 is fitted accurately with a cork, through which passes a glass tube about the diameter of a goose-quill, one end of which reaches to the bottom of the bottle, and to the other bent end is tied about 12 inches of india-rubber tubing of the same diameter. The glass tube is made perfectly air-tight in the neck of the bottle by sealing-wax varnish, and the india-rubber one is closed by a pinch-cock. In No. 2 neck is placed a simple contrivance which answers the purpose of a condensing syringe. It consists of a piece of glass tubing 5 or 6 inches in length fixed air-tight in the cork; to its upper extremity is attached a small india-rubber ball, having a small hole in one side, and to the lower end a small oil-silk valve, like those used in air-pumps, opening downwards. The third neck is closed by a cork, and serves to introduce the injecting fluids. To use the instrument, the proper sized nozzle is fixed on the india-rubber tube, which is closed by the pinch-cock. The requisite quantity of fluid is then poured into the bottle, and the cork firmly inserted. Pressure is now made on the india-rubber ball, taking care to close the hole with the finger. By this means air is forced down the tube through the valve into the bottle. On removing the pressure from the ball, the valve closes and the ball is re-filled through the hole in its side, and the compression can be increased to the necessary extent. The pinch-cock is now cautiously opened, and the fluid rushes up the tube completely filling it and the nozzle; the cock is then closed, preventing further exit, and the instrument is ready for use. The nozzle is introduced and tied into an artery in the same way as with the ordinary syringe. The inventor prefers glass nozzles to those of metal, as they are lighter, can be made very easily, and drawn out to very fine points. The instrument has the advantage of being self-acting, and leaving the operator the free use of both his hands, besides being more regular in its action than the usual syringe, saving in the hands of those well practised in its use. A form of apparatus nearly similar is employed by Dr. Rutherford; in this, however, the pressure is obtained by means of a column of water.

The subject of mounting objects in the dry way has received some attention from Mr. W. Ackland, F.R.M.S. Where thin cells are required, he employs a ring of the varnish already mentioned in this Journal (vol. ii., N.S., p. 271); this is allowed to become throughly dry before being used, a number of cells being made and kept in stock. Where thicker objects are to be mounted, metal or glass cells are used, coated on their upper surface with the same varnish. The cover is fixed by being clipped to the cell; heat is then applied, and the varnish softened. Upon cooling, the cover is securely fastened. Where from the nature of the object the slide cannot be safely heated, a thick disc of brass sufficiently heated is applied to the cover with the same result. For attaching delicate objects to slides, the following preparations are used:—

No. 1. Quinine Solution—

Sulphate quinine	5 grains.
Acetic acid	5 drops.
Water	1 ounce.

No. 2. Gelatine Solution—

Gelatine	20 grains.
Glycerine	5 drops.
Quinine solution as above ..	1 ounce.

No. 3. Gum Solution—

Gum tragacanth	5 grains.
Glycerine	3 drops.
Quinine solution	1 ounce.

A small portion of No. 2 or No. 3 solution is spread upon the glass before the cell is made. When the objects are to be attached to the glass they are placed on the prepared gum or gelatine surface, and the slide placed under a bell glass containing a saucer of water; this softens the coating, and attaches the minute objects. They are then dried under another bell glass, the moisture being absorbed by chloride of calcium. The covers are then fixed as before mentioned. The quinine solution may be used to prevent mouldiness in paste or gum, and is an advantageous substitute for the bichloride of mercury usually employed for this purpose, as the mixture is not poisonous.

Mr. F. H. Wenham has communicated to the Royal Society* the formulæ for the improved microscopic objectives recently constructed by him. His paper contains an account of the successive improvements in the construction of object-glasses. A little before the year 1829, the three superimposed achromatic lenses appear to have been in use; but no knowledge of their properties had as yet been arrived at, and it was not until Mr. J. J. Lister announced his discovery of the *aplanatic foci* to the Royal Society in the same year that any marked improvement took place. In the year 1831, the late Andrew Ross successfully constructed an object-glass on this principle, and at the same time made the discovery of the aberrations caused by covering the object with a film of glass, and applied the means for their correction. Mr. Wenham then described the increase of aperture obtained by the use of the *triple front* lens. This was followed by Mr. Lister's introduction of a *triple back* lens in the year 1850, by which the aperture of the ⅛th was increased to 130° or more, and this form was employed for high powers until quite recently. The next advance was Mr. Wenham's discovery of the feeble correcting power for colour of the *flint concave* in the triple front, and his successful substitution for it of a thick single lens, the form now usually employed in the best high-power objectives. His attempt was to substitute a single lens in place of the middle doublet. This, however, for reasons shown in his diagram, failed to produce the desired result, and led to the employment of a triplet between two single lenses. By this means perfect corrections were obtained, and the construction of the object-glass much simplified, there being only ten surfaces and but one concave of dense flint used in correcting four convex lenses of crown glass. Mr. Wenham in planning an object-glass prefers constructing a diagram on a large scale, as being far less intricate than mathematical calculation. The paper is very fully illustrated, and will be duly appreciated by all interested in the construction of objectives, as it forms an admirable sequel to the valuable series of papers by the same author in the first volume of the "Monthly Microscopical Journal."

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* Proceedings Royal Society, October 31, 1872.

HEAT.

Professor Wheildon, of Concord, U.S.A., advances, in opposition to what is known as the Gulf Stream Theory, an atmospheric theory to account for amelioration of climate and an open sea in the polar regions. The accounts of Arctic voyages show sudden rises of temperature when nothing but an unlimited extent of ice is near. These changes could not have been consequences of proximity of open water, which at the highest, would only be 29° of temperature. The theory of Professor Wheildon is that open, melting ice, rain after snow, and other phenomena in Arctic regions, are not caused by winds warmed by an open sea, but by a circulation of air in which warm winds descend from upper atmospheres; being a circulation by which winds heated at the equator reach the poles.

Having occasion to cool a red-hot copper ball, Mr. W. F. Barrett plunged it into a vessel of soapy water. The ball entered the water without any hissing or perceptible evolution of steam; and upon being removed seemed as brightly incandescent as before; other metal balls were then tried with the same result. The soapy water was then replaced by fresh; but upon plunging an incandescent ball into this, the hissing was loud and the evolution of steam copious. Mr. Barrett infers that the presence of soap in the water contributed to the formation of the spheroidal state. Further observation showed also that albumen, glycerine, and organic matters generally facilitated its occurrence. The author seeks to establish a possible relationship between this phenomenon and certain boiler explosions, from the possible entrance into boilers of oil or other organic matter.

The Earl of Rosse has communicated to the Royal Society a paper "On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its variation in amount with her Phases," in which he gives an account of a series of observations made in the Observatory of Birr Castle, in further prosecution of a shorter and less carefully conducted investigation, as regards many details, which forms the subject of two former communications ("Proceedings of the Royal Society," vol. xvii., p. 436; xix., p. 9) to the Royal Society. The observations were first corrected for change of the moon's distance from the place of observation, and change of phase during the continuance of each night's work, and thus a curve, whose ordinates represented the scale-readings (corrected), and whose abscissæ represented the corresponding altitudes, was obtained for each night's work. By combining all these a single curve, and table for reducing all the observations to the same zenith-distance was obtained, which proved to be nearly, but not quite, the same as that found by Professor Seidel for the light of the stars. By employing the table thus deduced, and also reducing the heat-determinations obtained on the various nights for change of distance of the sun, a more accurate phase-curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Prof. Zöllner's gives for the moon's light. By employing Laplace's formula for the extinction of light in our atmosphere the heat-effect in terms of the scale-readings was deduced, and an approximation to the height of the atmosphere attempted. From a series of simultaneous measurements of the moon's heat and light, at intervals during the partial eclipse of November 14, 1872, when clouds did not interfere, it was found that the heat and light diminish nearly, if not quite, proportionally; the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half their amounts before and after contact with the penumbra.

ELECTRICITY.

M. Gramme's constant current magneto-electric machine is now applied to the purposes of lighting and electroplating. As our readers are aware, the arrangement of this machine permits of the obtaining of an electric current in one direction, perfectly continuous, and of great strength. Mr. Sabine, who has recently tested one of the smaller varieties of these instruments, states that it gave an electromotive force of 68 to 258 Minotti's cells with 60 to 230 turns of the handle per minute. The instrument in question was composed of 36 bobbins, each containing 180 yards of No. 40 copper wire, revolving before six magnets. One of these machines is shortly to be employed in the lighting of the clock-tower of the Houses of Parliament.

Nearly related to this instrument is that of M. Le Roux, for exhibiting a modification of Faraday's celebrated experiment with the copper-disc induction-apparatus. A disc of red copper, 15 c.m. in diameter and 2 m.m. in thickness, receives from a multiplying motion a rotary speed of 180 turns per minute as a maximum. This disc is arranged between two circular masses of soft iron, these masses being connected by a frame of soft iron, portions of the frame forming the core of four electro-magnets. The faces of the masses of soft iron thus acquire an opposite polarity. With this apparatus a bright spark can be obtained.

Mr. Willoughby Smith records a most interesting experiment relating to the conductivity of selenium, a metal of very high resistance. Mr. Smith took several bars of the metal, of from 5 to 10 c.m. in length and 1 to 1½ m.m. in diameter. Each bar was hermetically sealed in a glass tube, and had a platinum wire at each end for the purpose of connection. It was found that the resistance altered materially, according to the intensity of the light to which the metal was subjected. When the bars were fixed in a box with a sealed cover, so as to exclude all light, their resistance was at its highest, and remained very constant; but immediately the cover of the box was removed the conductivity increased from 15 to 100 per cent, according to the intensity of the light falling upon the box. Merely intercepting the light by passing the hand before an ordinary gas-burner, placed several feet from the bar, increased the resistance 15 to 20 per cent. If the light be intercepted with glass of various colours, the resistance varies according to the amount of light passing through. To insure that temperature in no way affected the experiment, one of the bars was placed in a trough of water, so that there was about an inch of water for the light to pass through. The results were the same. And when a strong light, from the ignition of a narrow band of magnesium, was held about 9 inches above the sealed tube, the resistance immediately fell more than two-thirds, returning to its normal condition immediately the light was extinguished.

Considerable excitement has prevailed in electrical circles, caused by a paper published by M. du Moncel, in "Comptes Rendus," as to the conditions of the maximum resistance. His conclusions—instead of indicating that, for a galvanometer to attain the best possible conditions of sensibility with regard to a circuit of given resistance, it should be necessary that the resistance of the magnetising helix should equal that of the exterior circuit—have shown that the conditions of sensibility admit of a much greater length of wire than would correspond to the resistance of the exterior circuit. His calculations and experiments prove the maximum to be obtained with helices presenting twice the resistance of the exterior circuit.

M. Benoit has found the initial resistance in steel and iron to be doubled at 170°; in silver, copper, and gold, at 255°; in platinum, at 455°. In alloys the increase is generally more feeble; in standard alloy, for example, the resistance is increased at 860° by only 0·3 of the value at zero. The coefficient of expansion was carefully taken into account.

Dr. Blake, of the San Francisco Academy of Sciences, announces the discovery of a current of electricity running north and south, at a distance of about 150 miles from the Pacific coast, along a belt of metallic deposits, serving as a conducting-chain.

TECHNOLOGY.

It is estimated that there from 20,000 to 25,000 persons in Europe daily engaged in the preparation of hair and the manufacture of felt hats, in which processes they are exposed to mercurial poisoning. M. Hilairet, in experimenting on this subject, impregnated skins with a neutral substance, as molasses, or dextrine, or sugar, then put them in nitric acid, and found that by the action of the nitrous and hyponitric acids thus developed, the hair underwent a change of structure corresponding exactly to that obtained by means of the solution of mercury in nitric acid.

Von Scherzer, who first introduced to Europeans a varnish made by the Chinese, by beating together fresh blood and quick-lime, and used to make wooden articles completely water-tight, states that he has seen in Pekin wooden chests, which have been varnished with it, and after a journey over Siberia to St. Petersburg and back, were still sound and perfectly water-tight. Baskets of straw, used for the transportation of oil, are made fit for the purpose by means of this varnish; it also gives the appearance and firmness of wood to pasteboard coated with it. Articles required to be absolutely impervious are varnished twice, or at the most three times, by the Chinese.

M. Hallwachs asserts that not only green, but red carpets also contain arsenic, particularly the brilliant dark reds now so much in vogue. Samples of these carpets burned with the blue arsenic flame, gave off the characteristic garlic odour. Enough colour to give a distinct arsenic reaction could be rubbed off with the finger. A solution in hydrochloric acid produced the usual greyish precipitate of metallic arsenic.

A few years ago an oil well was started near Cumberland, Maryland; but instead of striking oil, the pioneers came upon a gas chamber and penetrated it. The gas was ignited and continued burning. About a year ago, Mr. Haworth, of Boston, purchased the well, and obtained a patent for the manufacture of carbon. The gas is allowed to burn against soapstone plates, on which the carbon is deposited in the form of soot. Six hundred and sixty burners are now in operation, each burner consuming 8 cubic feet per hour. By a mechanical arrangement, the soot is scraped and deposited in large tin boxes about 3 feet long, 1½ feet wide, and 1½ feet deep; scrapers are passed along the soapstone plates every twenty minutes, and the boxes are filled on their fourth passage. A building, twice the size of the present one, is now in course of construction. It will have in use 1328 gas burners. The present consumption of gas amounts to about one-twelfth the whole quantity escaping from the well. The total consumption of gas by the burners of both buildings will be one-fourth of the whole. The carbon is generally used for the manufacture of ink.

The following is a description of the process for preparing alcohol from sawdust:—Into an ordinary steam boiler, heated by means of steam, were introduced 9 cwts. of wet sawdust, 10·7 cwts. of hydrochloric acid (sp. gr. 1·18), and 30 cwts. of water; after eleven hours' boiling 19·67 per cent of grape sugar was formed. The acid was then nearly saturated with chalk. Yeast was added after the saccharine liquid had cooled down to 30° C., and the fermentation finished in twenty-four hours. 26·5 litres of alcohol of 50 per cent at 15° were obtained quite free from any smell of turpentine, and of excellent taste. It appears that the preparation of alcohol from sawdust may be successfully carried on industrially when it is precisely ascertained what degree of dilution of acid is required, and how long the liquid has to be boiled to convert all the cellulose into sugar. Fifty kilos. of the sawdust yield 12 litres of alcohol at 50 per cent.

Messrs. Baerle, of Worms, have discovered soluble glass to be a valuable washing powder and detergent. Take 40 parts of water, at a temperature of 50° to 57° C., and 1 part of soluble glass; plunge the wool into the mixture, stirring it for a few minutes. Then rinse the wool in cold or tepid water; it will be found to be quite white and void of smell. Sheep also may be washed with the same preparation, care being taken to cover the eyes of the animal with a bandage, to perform the washing with the solution quickly, and to remove the surplus with tepid water. In the case of combed wool, it should be first steeped into the solution, and afterwards into another bath composed of 80 parts of water at 37° C., and 1 part of soluble glass. In this way the employment of soap or soda is not necessary. For laundry purposes a bath must be prepared over night with 20 to 30 parts of water at 50° to 57° C., and 1 part of neutral soluble glass; the linen is plunged into this bath and left until the following morning, when, after the bath has been re-heated with additional hot water, it is to be worked with a wooden stamp. The colour of the solution shows when the fabric is clean. The operation is completed by rising with a little soap; but it is well to pass the fabric again through a weak solution, consisting of 1 part of soluble glass to 50 parts of water at 45° to 50° C., and then to rinse in fresh water.

CHEMICAL SCIENCE.

A new burette has been lately used in Paris. It consists of an upright tube drawn out to a fine aperture below, like that of Mohr, and supported in the same manner. The opening at top is fitted with a perforated cork, through which plays a glass rod, reaching down to the bottom, and ground conically, so as to fit water-tight into the tapering delivery-end of the burette. A lateral aperture at the top serves to charge the instrument. This form is useful in working with solutions of permanganate of potash, or other reagents which attack the india-rubber which in Mohr's pattern connects the delivery-tube to the body of the burette.

MM. Samal and Berouson have recently patented a new method of bleaching animal textile fabrics, by means of a feeble solution of the sulphurets of sodium and potassium. These products remove the gum in preparing silk and in scouring wool. In the first case the bath should be boiling; in the second, the temperature of the alkaline sulphuret should not exceed 50° C. The more difficult it may be to remove the gum and prepare the silk, the less the solution should be sulphuretted; in some instances the protosulphuret may be employed. The aluminates of soda and potash have also been used in the same manner.

ERRATA.—P. 173, line 19 from bottom, for "*land*" read "*band*." P. 201, line 26 from top, for "*masses*" read "*maps*."

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

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PROCEEDINGS OF LEARNED SOCIETIES, &c.

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